Vol. XXI. No 2. PSYCHOLOGICAL REVIEW PUBLICATIONS March, 1914

THE Psychological Review

EDITED BY

JOHN B. WATSON, Johns Hopkins University

HOWARD C. WARREN, Princeton University (*Index*)

JAMES R. ANGELL, University of Chicago (*Monographs*) and

ARTHUR H. PIERCE, Smith College (*Bulletin*)

ADVISORY EDITORS

R. P. ANGIER, Yale University; MARY W. CALKINS, Wellesley College; RAYMOND DODGE, Wesleyan University; H. N. GARDINER, Smith College; JOSEPH JASTROW, University of Wisconsin; C. H. JUDD, University of Chicago; ADOLF MEYER, Johns Hopkins University; HUGO MÜNSTERBERG, Harvard University; W. B. PILLSBURY, University of Michigan; C. E. SEASHORE, University of Iowa; G. M. STRATTON, University of California; E. L. THORNDIKE, Columbia University

CONTENTS

The Mental and the Physical: H. C. Warren, 79.

The Theory of Two Factors: C. Spearman, 101.

On the Use of the Rotating Sector in Photometry: A. H. Pfund, 116.

The Duration of Attention: M. LeRoy Billings, 121.

The Effect of Size of Advertisements and Frequency of their Presentation: Edward K. Strong, Jr., 136.

Concerning Individual Differences in Reaction Time: F. L. Wells and V. A. C. Henmon, 153.

PUBLISHED BI-MONTHLY BY

PSYCHOLOGICAL REVIEW COMPANY

41 NORTH QUEEN ST., LANCASTER, PA.

AND PRINCETON, N. J.

Entered as second-class matter July 23, 1897, at the post-office at Lancaster, Pa., under Act of Congress of March 3, 1879.

Psychological Review Publications

EDITED BY

HOWARD C. WARREN, PRINCETON UNIVERSITY (*Index*)
JOHN B. WATSON, JOHNS HOPKINS UNIVERSITY (*Review*)
JAMES R. ANGELL, UNIVERSITY OF CHICAGO (*Monographs*)
ARTHUR H. PIERCE, SMITH COLLEGE (*Bulletin*)

WITH THE CO-OPERATION OF
MANY DISTINGUISHED PSYCHOLOGISTS

THE PSYCHOLOGICAL REVIEW

containing original contributions only, appears bimonthly, on the first of January, March, May, July, September, and November, the six numbers comprising a volume of about 480 pages.

THE PSYCHOLOGICAL BULLETIN

containing critical reviews, notices of books and articles, psychological news and notes, university notices, and announcements, appears the fifteenth of each month, the annual volume comprising about 480 pages. Special issues of the BULLETIN consist of general reviews of recent work in some department of psychology.

THE PSYCHOLOGICAL INDEX

is a compendious bibliography of books, monographs, and articles upon psychological and cognate topics that have appeared during the year. The INDEX is issued in April or May, and may be subscribed for in connection with The REVIEW and BULLETIN, or purchased separately.

Annual Subscription to Review and Bulletin, $5.00 (Canada, $5.15, Postal Union, $5.30); Review, Bulletin, and Index, $5.85 (Canada, $6.00, Postal Union, $6.15); Bulletin, Alone, $2.75 (Canada, $2.85, Postal Union, $2.95).

Current Numbers of the Review, 50c.; of the Bulletin, 25c. (special issues 40c.); of the Index, $1.

THE PSYCHOLOGICAL MONOGRAPHS

consist of longer researches or treatises or collections of laboratory studies which it is important to publish promptly and as units. THE PHILOSOPHICAL MONOGRAPHS form a separate series, containing treatises more philosophical in character. The MONOGRAPHS appear at irregular intervals and are gathered into volumes of about 500 pages, with a uniform subscription price of $4.00. (Postal Union $4.30.) Each series may be subscribed for separately.

The price of single numbers varies according to their size. Fifteen volumes of the PSYCHOLOGICAL MONOGRAPHS have been issued, and the first volume of the PHILOSOPHICAL MONOGRAPHS is in progress.

LIBRARY OF GENETIC SCIENCE AND PHILOSOPHY

A series of bound books issued as accepted for publication. The price varies according to the size of the volume. Two volumes of the Library have already appeared.

Subscriptions, orders, and business communications may be sent direct to the

PSYCHOLOGICAL REVIEW COMPANY
Princeton, New Jersey

VOL. XXI. No. 2 March, 1914

THE PSYCHOLOGICAL REVIEW

THE MENTAL AND THE PHYSICAL[1]

BY HOWARD C. WARREN

Princeton University

THE DOUBLE-ASPECT VIEW

The mind-body relation is the Wandering Jew of science. It is ever re-appearing when other issues are dead and buried. For centuries the same opposing interpretations of the relation between the mental and the physical have fought for recognition. We still find advocates of the two monistic theories, materialism and animism; more numerous are the champions of dualism in its two forms, interactionism and parallelism; some welcome the double-aspect view; while many in despair adopt the agnostic attitude.

The psychologist of today may be content to leave the broader aspects of the problem in the hands of the philosopher. He can scarcely avoid taking some attitude on the relation of consciousness to nervous activity. It is true the behaviorist urges us to limit investigation to the objective side. Yet few among us are ready to abandon the empirical study of consciousness. Fewer still would hark back to Thomas Brown and base psychology on introspection alone. The tendency today is to study mental phenomena in relation to the activity of the nervous system. Even the laboratory investigator, with his fine scorn for verbal jugglery, finds himself impelled to adopt some hypothesis, or at least to frame some concept of that relation.

[1] Address of the president, before the American Psychological Association, New Haven meeting, December, 1913.

At present interactionism and parallelism seem to preponderate. The majority of psychologists adopt one or the other. The claims of each standpoint have been ably upheld and combatted by many recent writers. Carl Stumpf[1] in Germany, C. A. Strong[2] in America, and William McDougall[3] in England are familiar instances.

Were it permissible to adopt the test of inconceivability we should find ample grounds for rejecting both interactionism and parallelism. To some of us the interaction of brain and consciousness would seem to imply that two incommensurables may possess a common factor. It is as though one should attempt to guide a motor car by talking to the steering wheel. On the other hand parallelism, as usually expounded, suggests that the chauffeur's hand is beside the wheel but does not grasp it: the chauffeur indicates the direction with a wave of his hand; the wheel, turning of its own accord, responds exactly.

Now we may grant that a theory of the universe is not to be demolished by a casual jest. And still we may not wish to undertake a logical refutation of any metaphysical theory. The very bases of logic are undergoing reconstruction, and the times are not yet ripe for a final world-view. The scientific psychologist, however, does not aspire to a vision of the naked ultimate; he seeks only to clothe the observed facts in a garment which will make them duly presentable.

I shall try to show, first, that the double-aspect view is the most satisfactory formulation of the mind-body relation; second, that this hypothesis enables us to extend the mechanistic interpretation, so fruitful in other fields, to the objective data of psychology; and finally, that it leads naturally to a two-aspect psychology—the psychology of introspection and the psychology of behavior.

Historically, the double-aspect notion has suffered in two ways: In the first place, it lacks a convenient name. And further, to understand what such a relation means we need a suitable analogy taken from every-day experience; whereas a satisfactory illustration of this sort has never been given. In

[1] Presidential Address, 4th International Congress for Psychology, 1896.
[2] 'Why the Mind has a Body,' 1903.
[3] 'Body and Mind,' 1911.

consequence of this double handicap the two-aspect view has been misunderstood and misrepresented. It has usually been identified with parallelism or with some form of monism. As Professor McDougall points out, it is neither. He calls it a form of 'identity-hypothesis.' It is rather a dualistic monism, —a *monodualism*, to name it in conformity with other theories.

To make this view clear we need an observable instance of some 'thing' exhibiting two aspects, inseparably bound together, yet manifest in two distinct ways. The classic illustration of concavity and convexity is inadequate, for concave and convex surfaces are both perceived in the same way.

The *surface-mass* relation of matter offers a better analogy, and helps to a clearer understanding of the mind-body relation. As data of experience the *surface* of a body and its *mass* or weight are perceived by two distinct senses. Divide a lump of earth or metal into as many parts as you will, your eye perceives only the outer surface of the parts—you never see the mass within. Heft it, and you feel only the weight—the surface is never a datum of muscular perception. Alter the shape as you will, the relations of the constituent particles are altered *pari passu* with the change of surface relations. Mass and surface change conjointly; they are inseparably bound together; they are two radically distinct aspects of the same thing.

This illustration enables us to conceive the possibility at least of the double-aspect theory. I grant that it *proves* nothing. It is nothing more than analogy. Yet it serves to picture the relation between nervous system and consciousness without supposing them to be two distinct things, either interacting on one another or separate and parallel. The analogy is doubtless imperfect. Mass does not correspond, character by character, to the mental aspect or inwardness of the world; surface does not correspond to the physical aspect or outwardness of the world. The figure simply makes the monodualistic relation comprehensible.

This is of no small importance, inasmuch as many writers have affirmed that the double-aspect view is absurd or meaningless. McDougall says: "A thing or being or process can appear

under two different aspects, can manifest itself in two different modes, only if and when both aspects are apprehended by the mind of some observer; either one observer must occupy the two standpoints successively, or two or more observers must apprehend it from the different standpoints. Now, in the case of the physical and the psychical processes which are said to be two aspects of one process, there is no such observer occupying the inner standpoint and apprehending the inner or psychical aspect of the real event, except in the altogether exceptional case of the introspecting psychologist. . . . These considerations seem to me to raise an insuperable objection to this form of the identity-hypothesis; namely, there is lacking, except in certain special cases, any observer occupying the inner standpoint."[1] Notice, first, the anthropomorphic 'observer' whom Professor McDougall insists on introducing; and second, the persistence of the old Kantian assumption that there *must* be a basal substance, a Ding-an-sich underlying the two aspects. Metaphysicians have demonstrated more than once the logical impossibility of reconciling dualism with monism. Yet one may venture to challenge the validity of any disproof which rests on apriori foundations. The knife of logic is not yet ground to sufficient keenness for cutting metaphysical knots. We cannot forget that the spherical form of the earth was disproved by the absurdity of imagining men head downward at the antipodes. Despite apriori compulsion the scientist may content himself with the inwardness and outwardness which he actually discovers among the data of experience; the problem of a tertium quid or Ding-an-sich may be left to some future generation of scientific philosophers. To theorize at present as to what the 'underlying reality' *must* be and how it *must* appear, would be like Columbus's attempt to draw a map of America before his first voyage.

Dualism offers us two alternatives: either the mind influences the physical world, or else it is an epiphenomenon and we are only conscious automata. But is not a third solution possible? Do the changes of mass in a body *produce* its changes of surface? Do surface changes *cause* the alterations in its mass relations?

[1] W. McDougall, 'Body and Mind,' pp. 157–59.

There is surely but one set of changes involved, though they may be regarded from two distinct standpoints. Just so it is conceivable that neural changes, viewed from another standpoint, are not the cause but the *external appearance* of mental changes.

In the surface-mass relation one aspect of the change is perceived by the eye, the other aspect by the muscle sense. Similarly, in the neuroconscious relation one aspect is objective —it is perceived from without; the other aspect is subjective —it is the conscious experience of the living organism itself. The parallelist errs in divorcing the two. The outwardness and inwardness of reality need not be regarded as two independent series, but as two manifestations of the same series. There is a boot for each leg. Mind is no more an epiphenomenon of brain activity, than the brain center is a hypophenomenon of consciousness. It is clear that we are not reduced to a choice between the causal relation and absolute separation. Changes of surface and changes of mass do not influence one another, neither are they independent. Just so the monodualist regards the activity of consciousness and the activity of the nervous system as neither causally related nor parallel. They constitute one single process, observable in two ways.[1]

This view is in harmony with the potential energy theory proposed by Professor Montague in a recent paper.[2] If he regards consciousness as the *inner aspect* of potential energy, then he merely adds a limiting clause to the double-aspect theory. The restriction of consciousness to *potential* energy may admit of empirical verification or disproof at some future time. If on the other hand Dr. Montague believes that potential energy is *another name* for consciousness—that the two are identical—his assumption seems like identifying visual surface with the mass which we lift.

[1] An 'aspect' or *way of regarding phenomena*, does not affect the succession of the phenomena themselves; so that the influence of one aspect on another has no significance for our problem. Neither is it essential to determine which aspect represents the world the more faithfully. The present writer has found the surface-mass analogy most helpful in clarifying his own world-view. It has stood the test for at least 15 years.

[2] W. P. Montague, 'Consciousness a Form of Energy,' Essays Philosophical and Psychological in Honor of William James, 1908. (Cf. W. Ostwald, 'Natural Philosophy,' p. 78; Holt, 1910.)

The double-aspect theory simplifies the genetic problem of consciousness. We need not speculate at what point in the animal series or in the human embryo consciousness arises. For we can assume that consciousness or its prototype pervades the whole organic world. If consciousness is not a term in the series of physical events, then the fact that amœba and other lower creatures act mechanically and uniformly is no ground for denying that they possess consciousness.

It has been assumed that human consciousness exists merely in connection with the higher centers. This means only that the 'consciousness wherewith we are conscious' (that is, our personal stream of thought) is limited to cortical activity. A broader view of mentality is obtained when we posit an experience side corresponding to the lower centers as well. True, this is no part of our own experience; but neither do all the activities of the *higher* centers result in experiences which join up with our personal life. The pathological states of split-off consciousness are by no means the only examples in point. There are instances in normal life as well. We remember the strokes of a clock after it has ceased striking, though the impressions failed to reach our consciousness at the time. We solve a problem in our sleep or while attending to something else. All we can actually affirm in such cases is that the experience in question does not form part of our personal stream of consciousness. To deny that there is *any* consciousness connected with this cerebral activity is on a par with denying consciousness to our best friends because their experiences are not part of our own consciousness.

To each one of us the inner aspect of the world includes only that which himself experiences. The same induction which leads us to attribute consciousness to other human beings points to its existence in lower animals and in the lower centers within our own body. If consciousness is the selfhood of the organism, it may well be present in lower species and in the lower centers of man.

We should avoid an anthropomorphic interpretation of this lower selfhood. The consciousness of amœba and the consciousness of a spinal center are surely very different from

the consciousness of an organized human individual with his complex cortex and web of association fibers. To endow the simple protozoan with a consciousness similar to ours is absurd. It is like the uncritical attitude which attributes human rationality to trained horses, in defiance of all experimental evidence concerning their learning processes. We can only assume in amœba and his fellows some inner mental life of a very simple sort, which with the evolution of organic life gradually evolves in complexity into the human type of consciousness.

If the term 'consciousness' seems to imply too much intelligence, let us call the simplest forms of inner mental life *proto-esthesia*, and reserve consciousness for the higher phenomena. The essential point is to avoid assuming a fictitious break in the chain of mental evolution. Referring again to our analogy, the observer finds no division of material objects into masses with surface and masses without surface. Similarly, biology and animal psychology have found no dividing line in the organic scale between animals with consciousness and those which lack it. As in many other phases of nature there seems to be no real discontinuity—only a gradual evolution.

Professor Holt's[1] theory that the simplest data of human experience may be resolved into more primitive mental atoms, offers a satisfactory corollary to this view. The increasing complexity of consciousness year by year in the human child and the evolution of nervous complexity in the animal series are matters of observation. They strongly suggest that the initial form of sensation ('proto-esthesia') is something simple and undifferentiated. Professor Holt carries the notion of mental evolution to a rigorous conclusion. His view lends itself better to the double-aspect interpretation than to interaction or parallelism. In a recent paper Professor Herrick concludes that cortical activity consists in the 'coördination and integration of highly elaborated subcortical organic circuits';[2] this lends support to Professor Holt's view from the physiological side.

[1] E. B. Holt, 'The Place of Illusory Experience in a Realistic World,' *The New Realism*, especially pp. 350–355.

[2] C. J. Herrick, 'Some Reflections on the Origin and Significance of the Cerebral Cortex,' *Journal of Animal Behavior*, 1913, *3*, 236.

The Mechanistic Interpretation

One objection to a non-causal view of the mind-body relation demands special consideration. It is charged that unless interaction occurs between mind and body the universe is reduced to pure mechanism. If conscious activity has no effect on the sequences of the physical world, if thought is not a cause of action, then (it is argued) the living, thinking organism is an automaton and ethical responsibility becomes a myth.

Let us face the issue squarely. Scientific research points more and more in the direction of a rigid, mechanistic interpretation of the physical universe. It is time for scientists to recognize that the data of psychology conform to a uniformitarian pattern also.

Hitherto mental phenomena have been too often regarded as something apart from the world-order—something out of harmony with its regularity. In Newton's day the movements of the heavenly bodies were considered something apart from the movements of terrestrial bodies. The burden of proof and the fear of anathema rested on those who sought to harmonize them. Now the complexion of the world has changed. Chemists and physicists have demonstrated the uniformity and regularity of events in their domain. The doctrine of conservation is only one striking example. Physiologists are extending the same notion of uniformity to the more complex realms of cytology and organic processes. Animal psychologists have begun to trace out uniformities in the activities of organisms. Sociologists have discovered many regularities in the interaction of living creatures. All these sciences have done their best work under the inspiration of the uniformity hypothesis. If the glory of modern science is its determinism, why should uniformity be considered degrading in the sphere of mental events alone? If the 'reign of law' means automatism, should not the term automatism be purged of its stigma? Is psychology to remain under the imputation of holding a primitive conception of the world from which the other sciences are fast escaping? Must we not amend the traditional interpretation of choice, reason, and volition, so as to bring these phenomena into harmony with the scientific conception of uniformity in nature?

Our latest psychological text-books indeed are free from the older anthropomorphic misconceptions. They analyze the rational and volitional processes into elementary data. Yet they generally fail to explain the standpoint which their analysis implies. Upon examining carefully ten typical American treatises on general psychology which have appeared within the past decade I cannot find in any of them a definite statement of the attitude of modern psychology toward the traditional view of choice, reasoning, and volition.[1] The writers do not make clear that we have ceased to regard the higher mental processes as arbitrary, indeterminate phenomena. It behooves those among us who accept a uniformitarian world-view, to proclaim their attitude in no uncertain terms.

In order to emphasize the automatic character of activity in the lower organisms, biologists are wont to declare that their tropisms do not manifest any signs of 'choice.' According to Jacques Loeb,[2] "We are here dealing with a forced reaction in which the animals have no more choice in the direction of their motion than have the iron filings in their arrangement in a magnetic field." With becoming scientific courtesy our biological friends accept the notion of choice prescribed by traditional psychology. With equal scientific candor they rule this notion out of their own sphere. Is it not time for psychology to indicate plainly that it no longer regards choice as something arbitrary or indeterminate? In the early days of Darwinism it was urged against Natural Selection that the process is really not selection at all. Yet when we stop to consider, is not natural selection a *most typical* example of choice? The choice of nature is not arbitrary: it is determined uniformly by natural processes, yet it results in the weeding out of some alternatives and the establishment of others. This, in the opinion of uniformitarians, is the highest type of selection.

[1] The works referred to are Angell, 'Psychology,' 1904 and 1907; Calkins, 'First Book in Psychology,' 1909; Dunlap, 'System of Psychology,' 1912; Judd, 'Psychology, General Introduction,' 1907; Meyer, 'Fundamental Laws of Human Behavior,' 1911; Pillsbury, 'Essentials of Psychology,' 1911; Read, 'Introduction to Psychology,' 1911; Thorndike, 'Elements of Psychology,' 1905 and 1907; Titchener, 'Text-book of Psychology,' 1909–10; Yerkes, 'Introduction to Psychology,' 1911. The authors selected are all members of this Association.

[2] J. Loeb, 'The Mechanistic Conception of Life,' p. 218.

The burden of proof today should rest on those who question uniformity in any sphere. Among biologists the debate is between mechanism and vitalism. Can vital phenomena be wholly explained in physical and chemical terms or not? The term *vital force* may be employed in two different ways: either to indicate our *ignorance* of certain physiological processes, or to designate a sort of process quite *distinct* from physical or chemical change. In the former sense it has no bearing on the dispute. In the latter sense it presents a definite problem for investigation. Now what is known about vital forces? The advocates of vitalism claim only that certain vital phenomena are *not fully explicable* in physicochemical terms. They have no alternative explanation to offer—only the name *Entelechy*, which serves to label the unsolved problems but does not advance one whit our understanding of them.[1] Driesch, for example, brings forward three separate proofs of vitalism;[2] every one of these rests on the *inconceivability* of imagining a machine so constructed as to perform certain processes observed in organisms. This is the inconceivability of antipodes over again. A century ago it was inconceivable that the chemical composition of distant stars should ever be known; and it is not many years since the earth's curvature rendered the notion of radiotelegraphy absurd. Mechanism is not synonymous with artificial machinery. Any process which involves only physicochemical changes is mechanistic, though it differ radically from a linotype or a refinery.

The organic processes are every year brought more and more within the domain of physics and chemistry.[3] Unless some new force is definitely discovered, should not the scientist assume that the unexplained processes harmonize with those already worked out?[4] Vitalism is a *possible* hypothesis, but it is not scientifically acceptable, for it stands without direct support. The present evidence from biology justifies the

[1] Cf. H. Driesch, 'Science and Philosophy of the Organism,' Vol. 1, p. 143: "But shall we not give a name to our vitalistic or autonomous factor *E*, concerned in morphogenesis? Indeed we will . . .. Let that factor . . . be called entelechy."

[2] *Op. cit.*, Vol. 1, pp. 146, 224; Vol. 2, p. 71.

[3] See J. Loeb, *op. cit.* especially p. 14 and p. 26; B. Moore, 'The Origin and Nature of Life,' *passim*.

[4] Loeb, *op. cit.*, p. 59.

assumption that physicochemical processes govern the growth and activity of organisms,—that uniformitarianism and mechanism are interchangeable terms.

Psychology should adopt the same attitude toward the phenomenon of choice. With the uniformities in behavior of simpler organisms staring up at us from below, and the social uniformities of human groups staring down at us from above, are we not bound to work under the assumption that human choice is fully determined by natural antecedents? The physical antecedents and consequents which constitute the outer aspect of choice are matters of objective study.[1] Only when these processes become *too complex* for practical observation, do biologists and animal psychologists admit the existence of choice in the traditional sense,—that is, as indeterminate. May we not suspect that they conjure a name to cloak their ignorance of certain antecedents?

The same tendency to keep alive the folk-lore of prescientific psychology appears today in the popular notions of *reason* and *rationality*. In the older psychology reason was supposed to transcend the natural world—to be a sort of intuition of truth vouchsafed to man alone. Even today reasoning is popularly regarded as yielding results superior to the events of nature or even running counter to them. Yet when we analyze the situation closely what does reasoning do but *follow the processes of nature?* Reasoning is correct only when it brings to consciousness the very results that processes of nature would themselves work out. As a mental process, reasoning is a special form of association. It differs from ordinary association in that it reaches conclusions which tally with the objective world. Any slip in adding a column of figures transforms the associative process from the rational or logical type to ordinary contiguity or similarity. It is vain for philosophers to assert that we cannot think of 2 and 2 as equivalent to 5. Any bookkeeper who has spent hours searching for an error of one cent in a trial balance knows that it is perfectly possible to think "2 + 2 = 5" time and time again. In other words, reasoning

[1] "Choice is a term based objectively on the fact that the organism accepts or reacts positively to some things, while it rejects or reacts negatively or not at all to others" (H. S. Jennings, 'Behavior of the Lower Organisms,' p. 330).

is valid, not when it transcends the uniform, mechanistic pattern of objective nature, but only insofar as it adheres exactly to that scheme.

This view of reasoning bears directly on the free-will problem. Suppose for a moment that voluntary choice *is* indeterminate. The highest type of volition, according to libertarians, is that based on reasoning. But the only valid type of reasoning, as we have just noted, is mechanistic. Hence, the more closely our voluntary choice follows the 'automatic' processes of nature, the higher it becomes. Hence, the effort of indeterminate volition as it perfects itself is to be rid of its own indeterminacy. Admitting, then, that indeterminate choice does exist, we find in the evolution of the organic world the following progress: (1) a stage of determinacy at the bottom, followed (2) by a supposed stage of indeterminacy or freedom of choice, this yielding (3) to the highest stage, which is determinacy once more. But why assume the second stage, which is contrary to all the trends of modern science? Is it anything more than a gratuitous assumption? Is not the hypothesis of indeterminacy a relic of the folk-lore stage of psychology? We are beginning to understand somewhat the physiological mechanism of control and inhibition.[1] The nervous integration involved in these processes depends on past and present stimulation. Are we not bound to assume that the mental aspect of voluntary activity tallies with the physical aspect?

The extension of the mechanistic interpretation to human choice and volition involves a re-interpretation of the ethical concept of responsibility. The moralist should relinquish his apriori notion of oughtness and gather his data from empirical sources; he should above all consult the alienist and child psychologist before attempting to construct the foundations of responsibility. The real science of ethics is concerned with the sense of responsibility which mankind *actually possesses*—not the hypothetical responsibility of a mythical man. The data of ethics are the actual motives of human action; science has no place for the paradox of forceless incitations.

[1] See C. S. Sherrington, 'The Integrative Action of the Nervous System.'

In all these cases of choice, reasoning, volition, responsibility, the difficulty is the same: the philosopher, the biologist, and the common man have not yet put aside the anthropomorphic psychology of earlier times. In our day it is no longer assumed that the movements of planets are guided by angels. We have ceased to regard gravity as a mystical substance. Our children soon abandon the idea that an automobile is propelled by a horse concealed within. Yet many persons still think of voluntary choice as determined by a man inside a man. They glorify chance by teaching a morality based on indeterminate activity. Let psychologists stand firmly against this anthropomorphic attitude and swing psychology into line with the other sciences. Let us emphasize the assumption that mental phenomena, including volition and choice, are uniform. Let us place the burden of proof where it belongs—on those who argue for an arbitrary element in choice, and on those who regard reason as a shrewd creature within us working counter to the universal processes of nature.

We need not include intelligence in this indictment, for the term seems already to have undergone a hopeful change of meaning. Animal psychologists find a measure of intelligence in the capacity of a creature to improve through experience; they no longer assume a man inside a dog to make him act intelligently. They study empirically the animal's progress in acquisition—his adaptation of behavior to changes in the environment, his growing facility in performance. The empirical science of animal intelligence appears to be gaining wide acceptance. It remains for human psychology to study in the same way the growth of rational intelligence in childhood.

The problem of teleology still towers forbiddingly athwart our path. In popular psychology foresight belongs exclusively to reasoning and intelligence. Intelligent acts and rational acts are said to depend on the consciousness of an *end in view*, which is to be accomplished by the activity; they are therefore characterized as *purposive* or *teleological*. Some years ago Professor Minot[1] summed up his theory of consciousness as follows: "The function of consciousness is to dislocate in time

[1] C. S. Minot, Presidential address before the A. A. A. S., *Science*, 1902, *16*, 1–12.

the reactions from sensations." His language seems to exclude sensation from consciousness. But despite this confusion his theory points out a possible biological interpretation of the relation of consciousness to purpose. Vision, smell—any distance receptor, in fact—may modify the reaction of the living creature to a subsequent contact stimulus.

Take a typical case. An out-fielder reaches up and catches a batted ball. Leaving for a moment the interpretation, what is the essential fact which we all recognize? It is that the player consciously *anticipates* the outcome of events. He begins to react to the contact stimulus before the contact stimulus occurs. In plain language, he foresees the contact. But foresight is not limited to man. Looking down the organic scale we find everywhere, even in exceedingly low forms of life, contrivances of structure and function whereby an animal can anticipate through behavior the outcome of physical events. The dog chasing the hare runs first in one direction, then turns as the hare turns, and so on till he catches his prey. Medusa moves its tentacles toward approaching food and is thereby prepared to draw the nourishing substance into its mouth. In almost every creature results are foreseen—events are prepared for—reactions to stimuli begin before the stimuli actually occur. That is, the same reaction may be a direct response to one sort of stimulus and a teleological anticipation of another. In the case of avoiding-reactions the contact stimulus is prevented altogether by the foresight which the distant-stimuli produce.

Why attribute the purposive reactions in man to a special 'prophetic insight' of consciousness, when the predisposition can be amply explained by the mechanism of nervous coördination? Every distant-stimulus furnishes anticipations of a possible future complex experience; it yields certain parts of the complex before the remaining parts present themselves. In the example cited the fielder sees the ball and begins to react before it touches his hand; here the visual stimulus outstrips the contact stimulus. When we see an axe strike a distant tree we wink before hearing the blow; in this case one distant-stimulus outstrips another. There is nothing hyperphysical in a dislocation of the order of sensations.

One complication calls for special consideration,—namely, when ideal factors enter into the situation; that is, when part of the experience is due to central stimulation. Psychological studies of association indicate how ideas are aroused by sensations; on the nervous side this means that higher or ideal processes are stimulated by lower or sensory processes. Either ideal or sensory activity may issue in a reaction through motor discharge. It follows, then, that a distant-stimulus may excite a sensory brain process, that this may lead to an ideal brain process, and that the latter may thereupon produce a reaction adapted to a future contact stimulus or to another distant-stimulus.

When we see the axe descend for perhaps the twentieth time, we 'attend' to the coming sound. When the fielder raises his hand to catch the ball he reacts to the visual stimulus plus the ideal data organized through repeated ideomotor experience. If past stimuli leave any trace in the nerve centers (any *engram*, to use Semon's term) the reaction may follow the restimulation of that trace as readily as it will follow a direct sensory stimulus. Take a more complex case. I start for the station to board a train; that is, there arises in my *present* consciousness the mental image of boarding the train or the image of some more remote activity, accompanied by an emotional state of considerable strength. The brain process which is the outer aspect of this very complex mental process issues step by step in reactions which anticipate the successive environmental situations.

Mere complexity should not becloud the issue. Every brain process, like every reflex activity, is presumably the result of physicochemical processes. The assumption of a mysterious intuition or 'psychic force' adds nothing to the mechanistic explanation, even when the latter is most fragmentary. Professor Minot and the interactionists go out of their way unnecessarily in assuming a special activity of consciousness to account for the dislocation of reactions from sensations. The nervous organization suffices to explain it. Distant-stimuli and central stimuli coöperate to bring about anticipatory reactions; foresight is but the conscious side of

this process. The phenomenon is *both* physical and mental. Foresight is no more a special perquisite of the mind than hindsight.

The genesis of purpose is explicable on scientific and mechanistic grounds by natural selection.[1] The old anthropomorphic teleology may be replaced by a natural teleology. The neural basis for anticipatory reactions originated through fortuitous variations; such reactions proved beneficial to the organism and were selected. Those creatures which are able to anticipate the outcome of events have a distinct advantage in the struggle for existence; those which fail to anticipate are most likely to vanish from the earth. It matters not whether the basis be inherited structure, as in the case of instinct, or adaptive function, as in trial and error learning. The purposive activity of the living organism may be adequately accounted for in objective terms, without an appeal to special attributes of consciousness. Natural teleology is a scientific corollary to natural selection.

If vital and mental phenomena are reducible to physicochemical terms, it does not follow that biology and psychology are any the less independent sciences. The very same occurrence may have several totally different meanings. Let me illustrate. A commander of infantry shouts, 'Charge!' and the whole company dash forward. In one sense the captain's cry is a mere matter of *acoustics* and the motion of his men a problem in *mechanics*. A full description of the occurrence will include also the *chemistry* of muscle. But to stop here in our explanation would be absurd. Biology very properly regards the organism as a unit; the *physiology* of vocalization and locomotion are genuine scientific problems. The same events take on a *psychological* meaning when treated as 'call' and 'response.' And again, as an instance of human interaction they belong to the province of *sociology*. Furthermore, if the charge takes place on a battlefield it may present a problem in *military science*, a problem in *economics*, and a problem in *jurisprudence*. The physicochemical description

[1] "Teleology, then, when brought to its stronghold, is a genetic outcome" (J. M. Baldwin, 'Development and Evolution,' p. 279).

merely explains the raw material; it does not touch the sociological situation at all. When an acoustic vibration generated in the larynx is regarded as a social signal, the elementary facts are grouped into higher complexes, and such synthetic units require a totally different description. Baldwin points out that these higher syntheses are new *modes* which reality acquires in the course of evolution.[1]

Consciousness and Behavior

That primitive notions of choice, reasoning, and foresight persist today among scientists, is largely due to the interaction theory and vitalism. The regularity of physical nature is too well understood to admit of anything but a mechanistic view of the material world. A vitalistic or idea-force hypothesis involves the assumption that non-physical forces modify physical events. Consciousness is the most obvious non-physical datum; and so the interaction hypothesis is adopted. The psychologist's analysis and synthesis of consciousness has not sufficed to clear away the popular misconceptions regarding mental phenomena. Even today many intelligent thinkers believe in occult mental influences, and base their world-view on the Mysterious.

Psychology itself is in a measure responsible for this. Our science has been too much dominated by introspective analysis. We need a more thorough investigation of the objective side of experience. Such writers as Watson[2] and Bechterew[3] would remedy the defect by abandoning altogether the study of consciousness. While we may not agree with Professor Watson's destructive critique, his arguments in behalf of the science of behavior are convincing.

Introspection needs checking from outside sources. Our so-called sensations are by no means pure sense data; they are

[1] Baldwin, 'The Origin of a "Thing" and its Nature,' Psychol. Rev., 1895, *2*, 551–573; 'The Theory of Genetic Modes,' Development and Evolution, 300–334. A later writer has elaborated Baldwin's view under the name of 'creative evolution.'

[2] J. B. Watson, 'Psychology as the Behaviorist Views It,' Psychol. Rev., 1913, *20*, 158–177; 'Image and Affection in Behavior,' *J. of Phil., Psychol., &c.*, 1913, *10*, 421–428.

[3] W. von Bechterew, Objective Psychologie, Leipzig: Teubner, 1913.

resultants of sense stimuli and internal stimuli working together. Few of us can effectively analyze our most common experiences. In the classic example of perceiving an orange it is by no means easy to pick out the simple sensations of color, odor, weight, etc. A taste factor is generally present in our perception of the untasted orange, and a systemic attribute of pleasure tinges the whole experience. The psychological dispute as to whether some 150 elementary color sensations exist or merely 3 or 4, illustrates the difficulty of separating by introspection the central from the sensory data in comparatively simple experiences. The recent debate on imageless thought is another instance in point.

The biologist may find his eye or his microscope a defective instrument of research, but at least he can determine within narrow limits the direction and amount of error. Introspection on the contrary is ever liable to confuse peripheral with central factors, whether in perception or in ideation, and only the careful training of a Titchener can avoid hopeless confusion. Mental phenomena are not more obscure and more uncertain than physiological phenomena. The fault lies rather in our complacent reliance on introspection. Despite James's warning most of us are apt to forget, when we analyze our own consciousness, that the analysis itself introduces new factors into the datum of observation. On this account, if on no other, we must check up our introspective results by objective study.

But there is a reason still more cogent for the study of behavior. Objective methods are essential to genetic investigation. Animal psychology is more hindered than helped by introspective interpretation. The proto-esthesia of amœba is radically different from any human experience. To interpret amœba's activity in terms of human *consciousness* throws no light on either consciousness or behavior. On the other hand, the study of amœba's *behavior* throws considerable light on the behavior of higher organisms, as Jennings and Loeb have shown. An examination of the gradual evolution of tropisms, reflexes, instincts, intelligent action, and rational volition enables us to understand far better than before the meaning of human acts and consciousness.

The hope of psychology in the near future seems to lie in the study of behavior. Behavior reveals the dynamic aspect more fully than introspection. Introspective psychology is largely a Linnæan system. It deals with established varieties of experience. Behavior psychology is Darwinian. It traces the growth of experience in the animal series. It supplies material for the study of mental growth in the child.

An autocrat of all the sciences might well decree that psychologists give up introspective study for a term of years and devote themselves to the investigation of behavior. Indeed, a beneficent tyrant might properly go further. The psychologist today needs a substantial grounding in physiology and biochemistry. To understand behavior we must understand the vital processes; and these in turn demand a knowledge of carbon compounds and colloid phenomena which few psychologists possess. How can we interpret attention or inhibition without a knowledge of physiology? What light is thrown on effort and fatigue by an understanding of the chemical changes in muscle! According to Huxley, "Psychology is inseparably linked with physiology."[1] Today he would doubtless add that physiology is inseparably linked with chemistry.

The double-aspect view welcomes the study of behavior, with its physiological implications. But it does not limit psychology to that side alone. Introspection is a legitimate field for exact investigation. With all its shortcomings, modern psychology has yielded many results of scientific worth. The volumes of James and Stout, of Wundt and Titchener, are prime contributions to science. In Part IV. of his 'Psychology,' Professor Yerkes brings together a striking list of psychological laws. Many of these generalizations would be ruled out altogether under Professor Watson's conception of the scope of psychology. We surely all agree that any *exact generalization* belongs to the domain of science. Introspection has already yielded exact results; an impartial tribunal should accept its credentials.[2]

[1] T. H. Huxley, Ency. Britan., 9th ed., Art. 'Biology,' Vol. 3, p. 679.

[2] See, among others, J. R. Angell's recent defense of 'subjective' psychology (PSYCHOL. REV., 1913, *20*, 255–270).

One question remains to be considered. If consciousness and behavior are both legitimate subjects for scientific investigation, do they constitute a single science? Or shall we distinguish between the science of consciousness and the science of behavior? Something may be said for either attitude. The dividing line between kindred sciences is vague and their demarcation is largely a matter of convenience. The double-aspect view would prefer to treat conscious processes and behavior processes as branches of a single science, inasmuch as they are two phases of the same events.[1]

This involves a new definition of psychology. We can no longer regard it as the science of conscious phenomena. On the other hand to define it with Watson[2] as the science of behavior, or with Pillsbury[3] and Meyer[4] as the science of *human* behavior, cuts us off altogether from the study of introspective data; it flings the whole classic psychology overboard. Professor Pillsbury's volume is all that the most thoroughgoing introspectionist could desire. But to accomplish this he makes human behavior synonymous with consciousness.[5] Professor Meyer, on the contrary, uses introspective data only to arrive at laws of nervous activity and behavior.[6]

A broader concept must be had if both aspects, behavior and consciousness, are to be included in psychology. Professor Marvin regards psychology as the study of 'that which controls reactions,'[7] but he makes no provision for the mental processes which accompany brain activity. Professor Dunlap

[1] In the PSYCHOL. BULL., 1906, *3*, 217–228, the present writer developed a classification of mental 'functions' on different lines from the traditional classes of 'structure elements.' It is there pointed out that 'quality change' which earlier writers call 'mental synthesis' is an important factor in consciousness, while in the physical world differences in quality vanish one by one as science examines the phenomena more closely.

[2] J. B. Watson, 'Psychology as the Behaviorist Views It,' PSYCHOL. REV., 1913, *20*, 158.

[3] W. B. Pillsbury, 'Essentials of Psychology,' p. 1.

[4] Max Meyer, 'The Fundamental Laws of Human Behavior.' His definition is implied in the title of the book.

[5] This is true also of W. McDougall, who defines psychology as 'the positive science of behavior,' 'Psychology' (Home Univ. Library), p. 38.

[6] See his articles in *J. of Phil., Psychol. &c.*, 1912, *9*, 365–371, and *Amer. J. of Psychol.*, 1913, *24*, 554–563.

[7] W. T. Marvin, 'First Book in Metaphysics,' p. 259.

defines psychology as *the study of experience*,[1] but unfortunately he restricts the meaning of 'experience' to 'consciousness.'[2] Of all available terms, 'experience' requires least change in connotation to cover both consciousness and behavior. Using 'experience' in this broad sense we reach a concise working definition: *Psychology is the science of individual experience.*

Experience implies something which comes to the individual from without; and this is precisely what the larger psychology investigates. Behavior is distinct from physiology. Behavior psychology is the science of those organic processes which bring the individual into relation with his environment, while physiology is the science of the organic processes as such. The physiological functions of nutrition, growth, regulation, reproduction, are distinct from the psychological functions of sensation, discrimination, language, reasoning. Carried over to consciousness the relation of organism to environment becomes the relation of self to not-self. All the phenomena of consciousness, save perhaps certain obscure feelings, are concerned with the relations of self to environment. The exception disappears if we assume that the feeling-tone of consciousness brings the apperceptive self into relation with the lower selves or lower centers of the organism. From the double-aspect standpoint, then, we may define psychology as *the science of the individual organism or consciousness, as related to its environment.*[3]

To sum up: Science is not yet ready to adopt a metaphysics of mind and matter. But some working hypothesis of the psychoneural relation is needed in order to fix the scientific status of psychology. The double-aspect view (monodualism) seems to fit the conditions best. This conception of the relationship between mental and physical becomes clear when we examine the analogous relation between surface and mass in our perception of material phenomena.

[1] K. Dunlap, 'System of Psychology,' p. 4.

[2] *Op. cit.*, p. 6.

[3] Under this definition the data of psychology may be studied in both ways; e.g., discrimination is both *selective reaction* and *sensation-difference*, language is both *expression* and *communication*.

If mental and physical activity are two inseparable aspects of one series of events, then the scientific assumption of uniformity or 'law' is extended from the physical into the mental sphere. The old anthropomorphic conception of choice and reason must be radically amended. In the light of modern science the presumption is that mental phenomena, including choice and reason, are as uniform as physical events. The burden of proof rests on those who deny the regularity and determinacy of human volition and human reasoning. Even teleology may be brought into line with the mechanistic processes of nature. Foresight is the conscious counterpart of purposive activity, and purposive activity is due to distant-stimuli which prepare responses to subsequent contact stimuli by means of a complex nervous mechanism; its beginnings are manifest far down the organic scale.

Psychology should embrace both the inner and outer aspects of experience. It is the science of the relations between the individual and his environment. These relations may be studied either objectively as behavior, or introspectively as events of consciousness. Behavior study is essential to an understanding of genetic problems; it serves also to check up the data of introspection. Introspective psychology has disclosed uniformities among mental events; it claims scientific recognition as a branch which contributes to a unified view of the world. Without consciousness there would be no scientific observation or generalization. Sense perception and the logical processes require analysis quite as much as the facts and values which they reveal. Science must study its instruments as well as its data.

THE THEORY OF TWO FACTORS[1]

BY C. SPEARMAN

CONTENTS

	PAGE
1. Object of the Paper	101
2. Data of Simpson and Thorndike	102
3. Theory of Two Factors	103
4. Older Methods of Verifying this Theory	105
5. Improved Method	107
6. Examination of the Theory by the Improved Method	110
7. Conclusion	112

1. *Object of the Paper.*—For the increasingly numerous workers in the field of individual psychology, it is always a notable event when a new investigation, such as Simpson's 'Correlations of Mental Abilities,' issues from the laboratory directed by Professor Thorndike.[2] The investigation is on the now familiar basis of mental tests and correlational coefficients. But the tests present the novel and interesting feature of having been applied to adults whose intelligence had been demonstrated by the sternest criterion, the struggle of life. These subjects included, on the one hand, an intellectual *élite* of 17 professors and advanced students of Columbia University and, on the other, a proletariat of 20 persons 'selected from men in New York City who had never held any position demanding a high grade of intelligence.'

The publication seems to have been made almost simultaneously with one by Dr. Hart and myself,[3] so that neither was able to profit by the other. This was unfortunate, since the two researches form essential complements to one another,

[1] From the Psychological Laboratory, University College, University of London.

[2] Columbia University Contributions to Education, Teachers College Series, No. 53, 1912.

[3] 'General Ability, its Existence and Nature,' *British Journal of Psychology*, Vol. V., p. 53, 1912.

the main value of Simpson's work lying in the empirical facts elicited, while that of the other consists in an improved method of treating such facts.

The present paper proposes to make good this deficiency. Simpson's data have been elaborated by the improved method. As will be seen, the not inconsiderable labor expended has met with its reward; it appears to have resolved a disquieting scientific discord into firm harmony.

2. *Data of Simpson and Thorndike.*—Their correlations are given in the following table. They were calculated by the ordinary Bravais-Pearson formula, and have not (in this table) been corrected for 'attenuation' or otherwise altered.

TABLE I

THE SIMPSON-THORNDIKE CORRELATIONS ('RAW')[1]

	Ebbinghaus Test	Hard Opposites	Memory of Words	Easy Opposites	*A* Test	Memory of Passages	Adding	Geometrical Forms	Learning Pairs	Recognizing Forms	Scroll	Completing Words	Estimating Lengths	Drawing Lengths
	1	2	3	4	5	6	7	8	9	10	11	12	13	14
1. Ebbinghaus test	...	98	94	79	62	91	71	54	78	88	55	42	33	25
2. Hard opposites	98	...	84	80	64	81	79	70	73	74	52	43	26	25
3. Memory of words	94	84	...	62	55	82	49	56	73	71	53	40	28	21
4. Easy opposites	79	80	62	...	57	52	68	53	42	56	45	29	38	48
5. *A* test	62	64	55	57	...	55	54	73	39	51	39	59	25	22
6. Memory of passages	91	81	82	52	55	...	53	57	59	66	54	31	28	19
7. Adding	71	79	49	68	54	53	...	45	39	47	51	57	17	25
8. Geometrical forms	54	70	56	53	73	57	45	...	35	49	54	56	25	25
9. Learning pairs	78	73	73	42	39	59	39	35	...	69	36	29	26	09
10. Recognizing forms	88	74	71	56	51	66	47	49	69	...	44	37	34	28
11. Scroll	55	52	53	45	39	54	51	34	36	44	...	31	19	27
12. Completing words	42	43	40	29	59	31	57	56	29	37	31	...	21	07
13. Estimating lengths	33	26	28	38	25	28	17	25	26	34	19	21	...	24
14. Drawing lengths	25	25	21	48	22	19	25	25	09	28	27	07	24	...

Most of the tests used were already well known and may be found in Whipple's Manual.[2] The 'Ebbinghaus,' 'hard opposites,' and 'easy opposites' are familiar to every psychologist.[3] 'Memory of words,' 'memory of passages,' and 'learning pairs' are memorizing performances. The '*A* test' and the

[1] *Ibidem*, p. 56.

[2] 'Manual of Mental and Physical Tests,' 1910.

[3] See Whipple, pp. 445 and 319.

'geometrical figures' are both what are usually known as 'cancellation tests.'[1] Nos. 13 and 14 both consist in discrimination of length of visual lines, but 13 uses the psychophysical method of 'reproduction,' whereas 14 apparently uses that of 'minimal changes.' The remaining titles, 'recognizing forms,' 'completing words,' and 'adding' sufficiently explain themselves.

3. *Theory of Two Factors.*—In evaluating the above table, it must be remembered how little scientific significance usually attaches to any single correlational coefficient considered by itself. Such a correlation almost always admits of indefinitely numerous interpretations. To eliminate this equivocality and penetrate down to the underlying truths, every correlation needs to be regarded in the light of all the others. Not the single correlations, but their inter-relation, is the vital matter. Simpson and Thorndike seem to have had this in mind, since they principally seek to verify a recent theory which claims to have discovered this inter-relation in the case of mental tests.

This theory is to the effect that all mental tests exhibit correlation additional to, and usually much larger than, that arising from merely casual causes, such as similarity between one test and another. This additional and larger correlation is asserted to be deducible from one of the oldest, most widely held, though, indeed, obscurely enough uttered hypothesis of psychology: the hypothesis, *that all the intellective activity of any person depends in some degree on one and the same general fund of mental energy.*[2]

That correlation of merely casual origin should be thus excepted is clearly indispensable. Without this proviso, nobody would be foolish enough to assign any fixed value to the amount of correlation between two tests. For by making these sufficiently alike, in other words, by sufficiently increasing

[1] See Whipple, p. 254.

[2] Strictly speaking, the theory is broader. It only maintains the existence of *some* general factor (or system of factors). But as this is believed to consist mainly in a general fund of energy, we will for greater concreteness and lucidity speak only of this energy in the present paper.

the elements common to the two, the correlation between them could obviously be raised to any extent whatever.[1]

Still, in practice the limitation of the theory by this proviso is far less than might have been expected, as will readily be seen by examining it in the case of Table I. The table happens to contain one of the few instances where previous investigations have found this casual correlation to be large; this is, between the two tests of 'cancellation,' Nos. 5 and 8, which differ solely in the shape and familiarity of the figures cancelled.[2] To agree with the theory, therefore, the correlation between these two tests should *not* be readily explicable by the general energy, but should be appreciably greater than required by the latter. Another case possibly concerned is that of the three memorizing tests; in previous investigations, somewhat similar performances have been occasionally reported to exhibit correlations with one another a little larger than was to be expected from the influence of the general energy alone.[3] The last and still more dubious case is furnished by the two discriminations of length. At first sight, these might well seem so much alike as almost to constitute the same test. But previous experiment has indicated that sensory discrimination is extremely dependent on the psychophysical method used, and is especially dominated by the vagaries of the method of 'reproduction.'[4] It remains an open question, therefore, whether the difference of method suffices to preclude any special correlation arising from the resemblance. Thus, out of all the 91 different correlations in Table I., only one ought certainly to be affected by the proviso, while four others have some moderate probability of being so.

The statement of the theory on page 113 contains another important qualification; the success of any intellectual per-

[1] It is hard to find excuse for many critics of the theory having completely overlooked this proviso. Not only was it explicitly stated, but several actual instances were adduced to illustrate and prove it (*B. Journ. Psych.*, V., p. 59). That common elements can produce special correlation is as undeniable as that they can cause 'transference of training'; the two cases are closely akin.

[2] The previous instance of two tests differing in this manner is described in *Brit. J. Psych.*, V., p. 73.

[3] *Brit. J. Psych.*, IV., p. 293; V., p. 75.

[4] *B. J. P.*, V., p. 74.

formance is said to depend on the general energy *in some degree* only. This indicates that there is a *second* factor in the person's success, namely, his *specific capacity for that particular kind of performance*. Hence, Professor Sancti de Sanctis has appropriately termed the theory that of 'two factors.'[1] In fact, it is just this doubleness of dependence—say the supporters of the theory—that has so long confused psychologists as to the real state of affairs.

The double relation may be found clearer by many readers when expressed physiologically. And, of course, the accord with physiology is itself an important evidence. According to the commonly accepted theory of cerebral localization of function, every mental performance involves an activity of some particular group of cortical neurons. To this the present theory adds, that the particular group of neurons needs more or less reinforcement by the energy of the whole cortex (or some even more extensive area).[2] Thus, the two factors in success are quite distinct; firstly, there is the state of the particular group of neurons, their development and organization; and secondly, there is the whole cortex. The former may be called the 'specific' factor, as it is specific to that particular performance. The latter constitutes the 'general' factor, since it is required for all performances.

As regards the correlations between mental tests, the theory of two factors maintains that the specific one merely contributes the usually insignificant 'casual' additions discussed on pages 103 and 104. With this small exception, the whole of the correlations between the tests is declared to be deducible from the general factor alone.[3]

4. *Older Methods of Verifying this Theory.*—Now, the paper on 'General Ability' quoted on page 101 showed that the current attempts to verify this theory on the actually observed facts suffered from grave defects.

[1] See Proc. of International Congress of Medicine, Section for Psychiatry, London, 1913; discussion on the paper of Hart and Spearman on 'Dementia.'

[2] It is impossible to enter here into the precise physiological mode of action of this general energy; but it does not appear to offer serious difficulties.

[3] Of course, this proposition—and, indeed, the whole paper—only applies immediately to ordinary tests of mental ability. It does not claim, for instance, to include correlations between tests of fatiguability.

These attempts were centered on the proposition that, if the correlations were really all due to one and the same factor, they should admit of being arranged 'hierarchically'; this means, in such a manner that the values are highest in one corner of the table (generally, the left top is chosen) and thence gradually diminish in both vertical and horizontal directions, the diminution of every column (or row) being in the same proportion.[1]

But this criterion of hierarchy has turned out a failure. Not, indeed, that it has been convicted of any error. But it has shown itself singularly open to arbitrary usage. Its essential fault lies in being strictly applicable only to cases where the hierarchy may be expected to hold good with absolute exactness. The decision as to whether any table of correlations *tends* towards permitting the hierarchical arrangement is left a matter of dogmatic assertion; those writers who are biased in favor of the theory always find such a tendency to exist; whereas those with the contrary bias invariably arrive at the contrary conclusion. For an instance, we need not go beyond our Table I.; here Thorndike and Simpson discover no such tendency towards admitting of hierarchical arrangement; on the other hand, to the present writer the tendency is clearly present.

This fault is the more disastrous, since an exact hierarchy—or, indeed, any other perfectly regular arrangement—cannot be expected under any circumstances whatever. Every actual measurement, psychical or physical, is liable to more or less error; and ordinary scientific measurements, where a limited number of persons or objects are taken as a representative sample of the entire class, are especially disturbed by the 'error of sampling' (the median magnitude of this disturbance is, as is well known, expressed by its 'probable error').

Occasionally, other methods besides the hierarchy have been employed. Some particular correlations have been selected from the table for combination into a mathematical function, and then the latter's value has been compared with

[1] For the original statement of this proposition, see Spearman, 'General Intelligence,' *Am. J. Psych.*, XV., 1904, p. 274.

that to be expected from the theory. But these attempts have been, if possible, even worse off; for they, too, have for the most part ignored the errors of sampling; in fact, the sampling error of the function would usually have been exceedingly difficult to determine. And in addition, these other methods have had the grave vice of depending on certain arbitrarily selected values, instead of resting fairly on the entire available data.

5. *Improved Method.*—The last two sections have indicated some of the misunderstandings of theory and difficulties of method with which Simpson and Thorndike had to contend, and through which their valuable work has so far been deprived of valid interpretation. Let us turn to the improved method offered by the paper on 'General Ability.'

In the first place, the consequences flowing from the theory of two factors have been deduced in a far more satisfactory manner than previously. The following argument, besides being mathematically rigorous, is simple enough for a schoolboy to understand.

Let a, b, p, and q denote any four mental tests, and g the hypothetical general fund of energy. Using the usual symbols, let r_{ap} denote the correlation between a and p, while r_{aq}, r_{bp}, r_{bq}, r_{ag}, and r_{pg} have similar meanings. And let $r_{ap \cdot g}$ denote the correlation that a would have with p if the influence of the variations of g were eliminated. We have, then, by Yule's well known formula for 'partial coefficients':

$$r_{ap \cdot g} = \frac{r_{ap} - r_{ag} \cdot r_{bg}}{\sqrt{1 - r^2_{ag}}\ \sqrt{1 - r^2_{bg}}} \qquad (A)^1$$

But by the theory, the correlation between a and p is wholly due to g, so that when r_{ap} is freed from the influence of g, the ensuing value $r_{ap \cdot g}$ must $= 0$; hence, by equation (A), $r_{ag} \cdot r_{pg} = r_{ap}$; similarly, $r_{bg} \cdot r_{pg} = r_{bp}$. Dividing the former equation by the latter, we get $r_{ag}/r_{bg} = r_{ap}/r_{bp}$, and similarly $= r_{aq}/r_{bq}$. Whence we arrive at the fundamental equation:

$$\frac{r_{ap}}{r_{aq}} = \frac{r_{bp}}{r_{bq}} \qquad (B)$$

[1] Udny Yule, 'Introduction to the Theory of Statistics,' p. 235.

Here, the hypothetical quantity g has been eliminated, and there remains a relation asserted to hold good—when all due allowances have been made—of the correlations between any four tests, a, b, p, and q. The proof is so short and clear, and the resulting relation is so astonishingly simple and definite, that the theory now at any rate possesses one of the most valuable characteristics in highest degree: the capability of being readily submitted to crucial quantitative verification.

Also, equation (B) is clearly consonant with the older criterion of 'hierarchy,' for when the equation holds good, and then alone, the table of correlations admits of arrangement in the hierarchical order.[1]

The equation may be illustrated from Table I. The symbols a and b may, for example, be taken as denoting our tests 2 and 9. Let p and q denote any pair out of the remaining tests. If the equation holds good, the correlations of 2 with any other two tests should be in the same proportion to one another as the correlations of 9 with the same two tests. All the correlations in question are reproduced in Table II.

TABLE II

		Test 2, Denoted by a in Equation (B)	Test 9, Denoted by b in Equation (B)
Test 1	denoted by p and q in equation (B)	98	78
" 3		84	73
" 4		80	42
" 5		64	39
" 6		81	59
" 7		79	39
" 8		70	35
" 10		74	69
" 11		52	36
" 12		43	29
" 13		26	26
" 14		25	9

The next step in our improvement is—*exact* fulfilment of the equation being *a priori* precluded by sampling errors and perhaps by casual correlation—to devise means of measuring the *general tendency* of Table II. to satisfy the equation.

[1] This is too obvious to need formal demonstration.

This purpose is served by the following reasoning. As we have just seen, in order that Table II. should satisfy equation (*B*), any two values under test 2 must bear the same proportion to one another as the two corresponding values under test 9. But this is equivalent to saying that the whole column under test 2 must be perfectly correlated with that under 9. If, on the other hand, the values in Table II. have no tendency whatever to satisfy equation (*B*), then the correlation between columns 2 and 9 must be zero.[1] *Hence, the required measure of the tendency to fulfil the equation* (*B*) *is given by the correlation between the two columns of correlations.* Of course, this is calculated just as easily as between any other two series of numbers. We may conveniently denote it by the symbol R_{ab}.[2] It turns out to have here the positive and extremely high value of +.84.[3]

Clearly, this value will remain unaltered by any, however arbitrary, rearrangement of the order of the tests in table, and it thus escapes a grave defect in the hierarchical method.

[1] A similar result ensues from the not unplausible hypothesis, that each performance depends on a randomly selected group of very numerous independent elements, and that the correlation between any two performances is due to some of the elements happening to be common to both groups. For it could easily be shown that under these assumptions the correlation (compensated for sampling errors) between any two columns will tend to equal the correlation (*un*corrected for attenuation) between the two performances from which the columns derive; in our notation, R'_{ab} will equal r_{ab}; and both will average little over zero.

[2] Formally expressed:

$$R_{ab} = \frac{S(\rho_{xa}\rho_{xb})}{\sqrt{S(\rho^2_{xa})}\sqrt{S(\rho^2_{xb})}}$$

where ρ_{xa} denotes the deviation of any value in the column under test 2 from the average of that column; ρ_{xb} has a similar meaning with regard to the column under test 2; and S denotes the sum of the values in the bracket.

It may be noted in passing, that the significance of R_{ab} is by no means confined to the present problem. In particular, its relations to r_{ab} are most interesting; the one may easily be positive when the other is negative, or vice versa. For many purposes, R_{ab} seems to be the more important value of the two.

[3] The question arises, whether or not the coefficients should be corrected for 'attenuation' before calculating R_{ab}. The matter is simplified by the easily demonstrable fact, that if R_{ab} is = 1 for the corrected coefficients, it must be so for the uncorrected ones also, and vice versa. And even if R_{ab} has any other value, it will not in general by appreciably modified by correcting for attenuation (the statement sometimes made, that the hierarchy tends to be much better for uncorrected than for corrected coefficients is erroneous). The correcting process has the disadvantage of greatly complicating equation (*C*); hence, it has been omitted here, as in the paper on 'General Ability.'

The third and final step in the present improvement of method is to compensate for the influence of the errors of sampling. The value taken by R_{ab} when thus compensated may be denoted by R'_{ab}.[1] The detailed calculations for Table II. are given in the appendix, in order to enable any one to make such calculations for himself. The value comes here to no less than +.91.[2]

6. *Examination of the Theory by the Improved Method.*—Let us now apply this new method in an absolutely straightforward manner to the data obtained by Simpson and given in Table I.

Take first the question of 'casual' correlation. We have seen (p. 104) that out of the ninety-one different coefficients in this table only one unquestionably should be too large to agree with equation (*B*) (or with the hierarchy), namely, that between the two cancellation tests (5 and 8). Four other coefficients, those between the three forms of memorizing (3, 6, and 9) and between the two modes of discriminating length (13 and 14), have some probability of being too large. As the amounts involved are insignificant as compared with the trend of the whole table, we may be allowed to dispose summarily of this small matter by the old rough criterion of hierarchy. In drawing up the table, evidently, some attempt has been made to approximate to the hierarchical arrangement; hence, any values too great to fit into the hierarchy

[1] The formula is:

$$R'_{ab} = \frac{S(\rho_{xa}\rho_{xb}) - (n-1)r_{ab}\overline{\sigma_{xa}\sigma_{xb}}}{\sqrt{S(\rho^2_{xa}) - (n-1)\overline{\sigma^2_{xa}}}\ \sqrt{S(\rho^2_{xb}) - (n-1)\overline{\sigma^2_{xb}}}} \qquad (C)$$

where the ρ's have the same denotation as before; $\overline{\sigma^2_{xa}}$ is the mean value of (probable error ÷ .6745)^2 while $\overline{\sigma^2_{xb}}$ and $\overline{\sigma_{xa}\sigma_{xb}}$ have similar meanings; n is the number of different values of x. For proof, see *B. J. P.*, V., p. 80.

[2] It should be noted that this compensation for the effect of sampling errors, like most corrections, is devised so as to make the result right *on an average*. Consequently, the individual values obtained for R'_{ab} may be expected to fluctuate equally above and below the *true* value (obviously, they must do this, if their mean square deviation from the truth is to be a minimum). In the extreme case that the true value approximates to complete unity, about half the individual values obtained for R'_{ab} ought therefore to be greater than unity, although of course no true value of R_{ab} could be greater.

should be distinguished by being larger than its four neighbors, that is, the four above and below it and to its right and left. Now, the correlation between the cancellations exceeds its neighbors by an average of no less than .22; those between the memorizings do so by .11; while that between the two length tests exceeds its only neighbor by .17.[1] Thus, all the anticipations of the theory are borne out by the actual facts. And I find that the more elaborate demonstration, on the basis of measuring the discrepancies of the coefficients from equation (*B*), only leads to the same result with greater emphasis.

After this confirmation concerning these casual discrepancies to be expected from equation (*B*), let us turn to the more essential matter of the general validity of the equation. To deal with this effectively, it is well first of all to get rid of any excessive masking influence to be expected from the 'casual' correlation. But on page 104, we learn that this can be achieved with tolerable completeness merely by eliminating one of the cancellation tests, or, more fairly, by pooling the two together. We have simply to replace columns and rows 5 and 8 in Table I. by their average. This results in the very slightly modified Table III.

The final and crucial business is to work out the correlation between each pair of columns in Table III., using the formula given in equation (*C*).

The difficulty, however, arises, that it is not always possible to determine this correlation (either R'_{ab} or R_{ab}) satisfactorily. The calculation of a correlation is, as every one knows, based on the deviations of the values in question from their own average. But should these deviations be small and the samp-

[1] This small excess, .17 only, shows that the use of the method of reproduction so dominates the character of test 14 as to render it almost completely different from test 13. This incidentally reconciles an old discrepancy of result; Thorndike, Lay, and Dean found too large a correlation between discrimination of weight and that of length to be attributable to any general factor (*Am. Jour. Psych.*, XX., 364); and this was regarded as arguing against the existence of any such factor, and as indicating instead an especial correlation between all performances belonging to the 'sensory level.' Hart and I suggested, rather, that some specific correlation had been produced between the two sorts of discrimination merely by using in both cases the very dubious method of reproduction. Above the suspicion is justified; this method proves actually to possess such a dominating character; whereas all signs of any 'sensory level' have now vanished.

TABLE III

THE SIMPSON-THORNDIKE CORRELATIONS AFTER POOLING TOGETHER THE TWO TESTS OF CANCELLATION

	Ebbinghaus Test	Hard Opposites	Memory of Words	Easy Opposites	Pooled Cancellation	Memory of Passages	Adding	Learning Pairs	Recognizing Forms	Scroll	Completing Words	Estimating Lengths	Drawing Lengths
	1	2	3	4	5	6	7	8	9	10	11	12	13
1. Ebbinghaus test	...	98	94	79	58	91	71	78	88	55	42	33	25
2. Hard opposites	98	...	84	80	67	81	79	73	74	52	43	26	25
3. Memory of words	94	84	...	62	55	82	49	73	71	53	40	28	21
4. Easy opposites	79	80	62	...	55	52	68	42	56	45	29	38	48
5. Pooled cancellation	58	67	55	55	...	56	49	37	50	46	57	25	23
6. Memory of passages	91	81	82	52	56	...	53	59	66	54	31	28	19
7. Adding	71	79	49	68	49	53	...	39	47	51	57	17	27
8. Learning pairs	78	73	73	42	37	59	39	...	69	36	29	26	09
9. Recognizing forms	88	74	71	56	59	66	47	69	...	44	37	34	28
10. Scroll	55	52	53	45	46	54	51	36	44	...	31	19	27
11. Completing words	42	43	40	29	57	31	57	29	37	31	...	21	07
12. Estimating lengths	33	26	28	38	25	28	17	26	34	19	21	...	24
13. Drawing lengths	25	25	21	48	23	19	29	09	28	27	07	24	...

ling errors relatively large, the former become swamped in the latter, and the calculation is illusive. In practice, therefore, it is necessary to fix some more or less arbitrary limit, at which R'_{ab} is so much disturbed by the sampling errors that its calculation shall be deemed no longer profitable. In the paper on General Ability, it was accordingly found advisable to reject all pairs of columns, in either of which the sum of the squares of the probable errors exceeded one fourth of the sum of the deviations from the average. If we retain this standard on the present occasion, there turn out to be 23 usable pairs of columns. The correlations, both raw and corrected, for each pair are given in the following table.

7. *Conclusion.*—Thus the mean value of the correlation between columns turns out to be no less than +.96. The discrepancy between this and unity—whether it be attributable to the still uneliminated casual correlation, or to mere chance—is of negligible magnitude.

The net result, therefore, of applying the new exact method in a perfectly plain and impartial manner to the experimental data of Simpson and Thorndike is a complete confirmation of the theory of two factors. The opposite conclusion arrived

TABLE IV

THE CORRELATIONS FOR EACH PAIR OF COLUMNS IN TABLE III

	R_{ab} Raw Correlation: $\frac{S(\rho_{ax}\rho_{bx})}{\sqrt{S(\rho^2_{ax})}\sqrt{S(\rho^2_{bx})}}$	R'_{ab} Correlation Corrected for Sampling Errors: $\frac{S(\rho_{ax}\rho_{bx})-(n-1)r_{ab}\sigma_{ax}\sigma_{bx}}{\sqrt{S(\rho^2_{ax})-(n-1)\sigma^2_{ax}}} \times \sqrt{S(\rho^2_{bx})-(n-1)\sigma^2_{bx}}$
1. Ebbinghaus test and hard opposites	+.94	+ .95
2. " " memory of words	+.85	+ .87
3. " " easy opposites	+.93	+ .98
4. " " memory of passages	+.90	+1.02
5. " " recognizing forms	+.88	+ .96
6. Hard opposites and memory of words	+.96	+ .99
7. " " easy opposites	+.96	+1.00
8. " " memory of passages	+.96	+1.14
9. " " learning pairs	+.67	+ .76
10. " " recognizing forms	+.95	+1.05
11. Memory of words and easy opposites	+.96	+ .92
12. " " memory of passages	+.96	+1.10
13. " " learning pairs	+.73	+ .86
14. " " recognizing forms	+.96	+1.05
15. Easy opposites and memory of passages	+.95	+1.08
16. " " learning pairs	+.70	+ .76
17. " " recognizing forms	+.95	+1.08
18. " " cancellation	+.78	+ .99
19. Memory of passages, learning pairs	+.74	+ .92
20. " " recognizing forms	+.95	+1.08
21. Learning pairs and recognizing forms	+.64	+ .75
22. Cancellation and memory of passages	+.69	+ .80
23. " " recognizing forms	+.73	+ .94
Average	+.86	+ .96[1]

at by these investigators themselves proves to have been merely an illusory product of the older defective methods, and to vanish on removal of the defects.

This ending cannot but be considered highly desirable for psychology. It shows the present data to be in perfect accord with all the other relevant facts hitherto published, whether by authors biased at the time in favor of the theory or against it. When writing 'General Ability,' these facts embraced the work of 14 experimenters on 1,463 men and women, boys and girls, sane and insane; the average correlation between columns throughout these amounted to +.97. During the brief period that has since lapsed, there has been added the research of Wyatt on 75 children, with a mean R'_{ab} of again +.97;[2] also, there is about to appear one by Abelson, where it

[1] As regards many of the values of R'_{ab} being greater than unity, see note 2 to p. 105.

[2] *B. J. Psych.*, VI., 1913, p. 109.

amounts to +1.02 and one by Webb, where it again comes to +1.02. Of contrary evidence, on the other hand, there has not ever been found to my knowledge one single instance.

The importance of this result is estimated by the present writer very highly. The bare fact of such an exact concordance throughout such a vast extent of independent research, including work of the standard of Thorndike's laboratory, seems to indicate that the science of psychology is entering on a new era. And the conclusion backed by such an accumulated mass of evidence, the theory of two factors, does not confine its significance to individual differences of ability, but —as the writer hopes to show shortly—reaches the heart of all the great problems of attention, association, fatigue, transfer of training, and mental growth. Its bearings are not even limited to the cognitive aspects of mind, but extend their ramifications deep into the affective and conative aspects also.

Appendix

The calculation of R_{ab} and R'_{ab}, exemplified in columns 2 and 9 in Table I.

Tests 2 and 9 are denoted by *a* and *b*. Their correlations with each of the other tests are written in the usual way as

Table V

Values Obtained from Columns 2 and 9 in Table I. for the Purpose of Calculating R_{ab} and R'_{ab}

1	2	3	4	5	6	7	8	9	10	11	12
r_{ax}	r_{bx}	ρ_{ax}	ρ_{bx}	c_{ax}	c_{bx}	ρ^2_{ax}	ρ^2_{bx}	$\rho_{ax}\rho_{bx}$	c^2_{ax}	c^2_{bx}	$c_{ax}c_{bx}$
.98	.78	.33	.33	.01	.04	.1089	.1089	.1089	.0001	.0016	.0004
.84	.73	.19	.28	.03	.05	.0361	.0784	.0532	.0009	.0025	.0015
.80	.42	.15	−.03	.04	.08	.0225	.0609	−.0045	.0016	.0081	.0036
.64	.39	.01	−.06	.06	.09	.0001	.0036	−.0006	.0036	.0081	.0054
.81	.59	.16	.14	.04	.07	.0256	.0196	.0224	.0016	.0049	.0028
.79	.39	.14	−.06	.04	.09	.0196	.0036	−.0084	.0016	.0081	.0036
.70	.35	.05	−.10	.06	.10	.0025	.0100	−.0050	.0036	.0100	.0060
.74	.69	.09	.24	.05	.06	.0081	.0576	.0216	.0025	.0036	.0030
.52	.36	−.13	−.09	.08	.09	.0169	.0081	.0117	.0064	.0081	.0072
.43	.29	−.22	−.16	.09	.10	.0484	.0256	.0352	.0081	.0100	.0090
.26	.26	−.39	−.19	.10	.10	.1521	.0361	.0741	.0100	.0100	.0100
.25	.09	−.40	−.36	.11	.11	.1600	.1296	.1440	.0121	.0121	.0121
Mean = .65	Mean = .45					Sum = .6008	Sum = .4820	Sum = .4526	Mean = .00434	Mean = .00726	Mean = .00538

r_{ax} and r_{bx}, and are given below in columns 1 and 2 respectively of Table V.[1] The deviation of each value of r_{ax} from the mean of r_{ax} is denoted by ρ_{ax} and given below in column 3; ρ_{bx} is analogous, and given in column 4. The 'probable errors' of r_{ax} and r_{bx} are denoted by e_{ax} and e_{bx}; they are given in columns 5 and 6. n is the number of different values of x.

$S(\rho^2_{ax})$, $S(\rho^2_{bx})$ and $S(\rho_{ax}\rho_{bx})$ denote respectively the sums of: the squares of ρ_{ax}, those of ρ_{bx}, and the products $\rho_{ax}\rho_{bx}$. They are given at the bottom of columns 7, 8 and 9. $\overline{\sigma^2_{ax}}$, $\overline{\sigma^2_{bx}}$, and $\overline{\sigma_{ax}\sigma_{bx}}$ denote respectively the mean values of e^2_{ax}, e^2_{bx} and $e_{ax}e_{bx}$, each divided by $.6745^2$; these mean values (anterior to the division by $.6745^2$) are given at the bottom of columns 10, 11, and 12.

Then, by the ordinary correlational formula, the correlation between columns 1 and 2,

$$R_{ab} = \frac{S(\rho_{ax}\rho_{bx})}{\sqrt{S(\rho^2_{ax})}\ \sqrt{S(\rho^2_{bx})}} = \frac{.4526}{\sqrt{.6008}\ \sqrt{.4820}} = .84$$

On correction for sampling errors, the correlation between the same two columns becomes (*B. J. P.*, V., p. 82),

$$R'_{ab} = \frac{S(\rho_{ax}\rho_{bx}) - (n-1)r_{ab}\overline{\sigma_{ax}\sigma_{bx}}}{\sqrt{S(\rho^2_{ax}) - (n-1)\overline{\sigma^2_{ax}}}\ \sqrt{S(\rho^2_{bx}) - (n-1)\overline{\sigma^2_{bx}}}}$$

$$= \frac{.4526 - 11 \times .73 \times \dfrac{.00538}{.6745^2}}{\sqrt{.6008 - 11 \times \dfrac{.00434}{.6745^2}}\ \sqrt{.4820 - 11 \times \dfrac{.00726}{.6745^2}}} = .91.$$

[1] Omitting the two which have no corresponding correlation in the other column.

ON THE USE OF THE ROTATING SECTOR IN PHOTOMETRY

BY A. H. PFUND

Johns Hopkins University

The rotating sector or episcotister has been so widely used in photometric measurements that the soundness of such application has been doubted no more than that of the law of inverse squares. It was somewhat surprising, therefore, that Parker and Patten[1] should have come to the conclusion that errors as large as 5.9 per cent. are introduced when a rotating sector is used to cut down the intensity of a beam of light. Were this instrument of little importance, the above paper might be passed over without comment. However, since we have nothing to take the place of the rotating sector, which is invaluable in photometry, it seems worth while to call attention to the experiments which justify the use of this instrument and also to point out the experimental errors which are responsible for the results of Parker and Patten.

The subject is an old one and deals with the verification of Talbot's[2] law which has been stated by Helmholtz[3] as follows: "If any part of the retina is exited with intermittent light recurring periodically and regularly in the same way, and if the period is sufficiently short, a continuous impression will result, which is the same as that which would result if the total light received in each period were uniformly distributed throughout the whole period." The experimental verification of this law has been carried out by a number of investigators.[4] As the work of Hyde[5] is the most thorough, a brief description of the

[1] Parker and Patten, *Am. Jour. Physiology*, 31, No. 1 (1912).

[2] Talbot, *Phil. Mag.*, Series 3, Vol. 5, p. 321 (1834).

[3] 'Physiolog. Optik.,' II. Auflage, p. 483.

[4] Plateau, *Pogg. Annalen*, 35, p. 457 (1835); Helmholtz, *loc. cit.*; Kleiner, *Pflügers Archiv*, 18, p. 542 (1878); Wiedemann u misserschmidt, *Wied. Annalen*, 34, p. 465 (1888); Lummer u. Brodhun, *Zs. für Instr.-kunde*, 16, p. 299 (1896).

[5] Hyde, Bull. Bureau of Standards, Vol. II., p. 1 (1906).

principle of his method will be given. Two similar Nernst lamps are mounted on the arms of a photometer bench and their distances are so adjusted that a photometric balance is established in a Lummer-Brodhun photometer. If now the light from one of the sources be cut down to ¼ of its original intensity by means of a rotating sector it will be necessary to double *exactly* the distance of the second source in order that a photometric balance be retained, *i. e.*, provided Talbot's law holds. By proceeding in this manner, taking into account a slight correction for the finiteness of the light source, Hyde carried out an exhaustive test of Talbot's Law for white light for all angular openings between 288° and 10° and found that the law was verified to within a possible error of ⅓ of 1 per cent. which probably expresses the limit of accuracy of the experiments. The law was found to hold equally well for red, green and blue light. It is therefore established by methods which are entirely free from objections, that Talbot's law holds to a high degree of accuracy for the human eye.

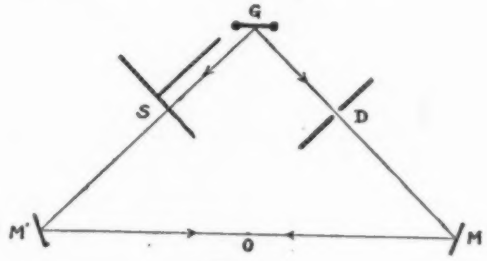


FIG. 1.

The apparatus used by Parker and Patten to test Talbot's law is represented diagrammatically in Fig. 1, where *G* is a Nernst lamp whose radiations along *GSM'O* are weakened by a rotating sector *S*, while those along *GDMO* are weakened by a diaphragm *D*. Visual balance of the two beams is first established by means of a Lummer-Brodhun photometer at *O* and this is subsequently replaced by a radiomicrometer which serves to measure the "physical intensity" of the two beams inividually. If the deflection produced by the energy of the one beam is not equal to that produced by the other then

Talbot's law is supposed to be invalid. Now the principal objection to this method of procedure may be formulated by stating that, while the human eye responds only to the visible radiations, the radiomicrometer responds to the entire range of wave-lengths emitted by the Nernst lamp, *i. e.*, the deflections given by the radiomicrometer are not at all a physical measure of the visible radiations, but of all of the radiations. Considering that the visible radiations form less than 4 per cent. of the total radiant output of a Nernst lamp, it is rather important to see what becomes of the other 96 per cent. which lie in the infra-red and which have by far the greater effect on the radiomicrometer. It is obvious that the diaphragm *D* cuts down the intensity of the light reaching *O* by limiting the effective length of the luminous Nernst lamp glower *G*, as is shown in Fig. 2. If the glower had the same temperature and

FIG. 2.

hence the same luminosity along its entire length, the method would be sound—but such is not the case. Due to conduction along the lead wires, the temperature and luminosity near the ends of the glower are markedly less than at the centre and as the diapragm limits the effective length of the glower to the central portion, this alone is capable of sending its radiations over the path *GDMO*. If we were to plot the energy curve of the central portion of the glower and also that of an equal length near the end of the glower, we should obtain the curves *A* and *B*, respectively, shown in Fig. 3. Since the maximum of an energy curve is shifted toward the shorter wave-lengths as the temperature of the source is increased, it is obvious that the amount of energy, for curve *A*, lying within the visible spectrum, is the larger. In order to bring about a photometric balance it is necessary to reduce the effective length of the glower by means of the diaphragm until the areas of the two

curves bounded by the confines of the visible spectrum are the same. But in doing this, all ordinates are reduced in the same proportion, as is shown in curve *A'* which shows the *same* amount of visible radiation as curve *B* but *less* infra-red. The eye responds only to visible radiations and, according to the eye, equality has been established. The radiomicrometer, however, records deflections which are proportional to the areas *VBRB* and *VA'RV*. As these areas are different, the deflections will be different even though the amounts of visible radiations be the same.

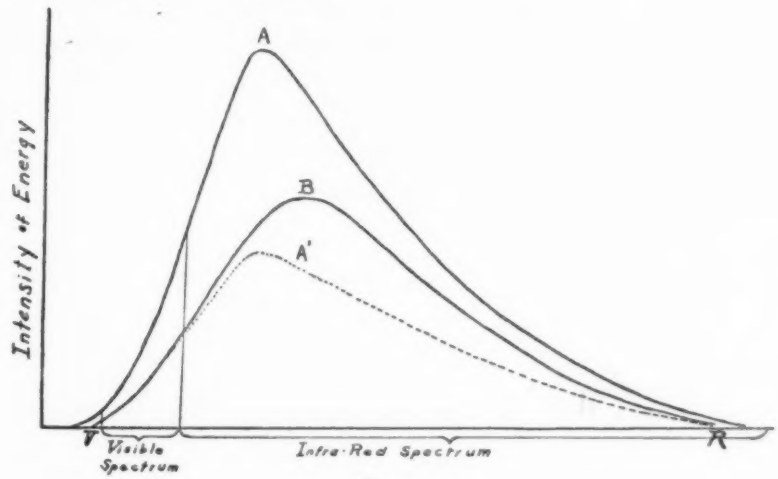


FIG. 3.

This discussion may be applied at once to the method of Parker and Patten. As has been stated, while the diaphragm *D* permits only the radiations coming from the central or hottest part of the glower to reach *O*, the rotating sector permits the radiations from the entire glower to pass on. Now the mean temperature of the entire glower is less than that of the central portion, hence, if a visual balance be established, we shall have conditions entirely similar to those shown in Fig. 3 where curve *B* refers to the energy curve of the radiations transmitted by the rotating sector and curve *A'*, to that of the radiations transmitted through the diaphragm. It is clear that we should expect the radiomicrometer deflections due

to the radiations coming through the diaphragm to be the smaller—which is precisely the result found by Parker and Patten (page 27, *loc. cit.*).

It may likewise be shown with equal ease, that if the width of the diaphragm be so adjusted that the two beams give the same radiomicrometer deflections, then the visual intensity of the beam transmitted by the diaphragm will be the greater (page 28, *loc. cit.*).

In conclusion it may then be stated that the use of the rotating sector has been fully justified by experiments which are entirely free from objections and that the experiments of Parker and Patten in no way cast doubt upon the validity of Talbot's law.

November, 1913.

THE DURATION OF ATTENTION

BY M. LEROY BILLINGS

(From the Psychological Laboratory of the University of Michigan)

Before entering into a discussion of my topic I wish to acknowledge my indebtedness to Professor W. B. Pillsbury, with whom I did this work, to Dr. John F. Shepard for his many helpful suggestions, and to Dr. E. C. Rowe for reading and suggesting changes in the manuscript. I also wish to thank those who acted as subjects for the experiments.

The many estimates in regard to the duration of attention have varied from a few seconds, as given by Professor Pillsbury in his 'Essentials of Psychology,' page 122, to possibly two or three hours as given by Professor Titchener in his 'Text-book in Psychology,' page 291. In this paper the author hopes to substantiate the opinion of the former on the subject by giving the results of a series of experiments carried on while in the University of Michigan in 1910.

The material used for observation was different from that hitherto used in such experiments and the method is more direct. Instead of using faint lights or liminal differences for observations we used common objects of ordinary intensity or memory images. The stimuli used were dots on paper, small parts of pictures, cutaneous stimuli of various sorts, the noise of a buzzer, etc. The subject was asked to press upon a key so long as the stimulus in question occupied the attention and to lift his finger whenever attention wandered to any other object or idea. He was later asked to enumerate the objects he observed in the interval as well as to record the time during which the attention could be held. At first the subject had difficulty in recording the moment of the wandering. He would find himself thinking of something else but would not have noticed when he wandered away from the stimulus he was supposed to observe. This source of error was probably present throughout the experiments but diminished with

practice. The results record the time our subjects could attend to a supraliminal stimulus without permitting any other object or idea to occupy the field of consciousness. The objects used for observation are given in the tables.

The experiments were performed as follows: Two rooms were used, one for the apparatus and one for the subject. In the apparatus room was a revolving drum, driven by an electric motor, upon which was placed smoked paper. An electric marker recorded upon this drum the fluctuations of attention as observed by the subject in the adjoining room. The subject pressed a button whenever he noticed a change and this marker recorded it upon the smoked paper. The time was marked off by a time-marker. The room in which the subject sat was a dark room, illuminated by artificial light. The only purpose of this was to get a tight room so as to exclude as many disturbing noises as possible. There were in this room a bell and a buzzer that could be set going by the operator at any time by pressing a button. There were also two copper electrodes placed on the subjects' arm by which the subject could be given an electrical stimulus, regulated by the operator. These last were used only as distracting stimuli to determine the effect upon the duration of attention.

The subjects for these experiments were Dr. John F. Shepard, assistant professor in psychology in the University of Michigan, Dr. Woodrow, assistant in psychology in Columbia at the time these experiments were performed, and Messrs. Harry W. Crane, R. Thane Cook, John E. Winter, and Wm. B. Fullerton, all of whom, except Mr. Cook, were graduate students taking special work in psychology.

The detailed tabulations of separate experiments contain mainly the experimental findings with Dr. Woodrow and Mr. Fullerton. The reason for choosing these two is that they served as subjects throughout the experiments while Messrs. Crane, Cook and Winter served for short periods. The variety of stimuli used with the latter subjects was also much less. As to the results obtained from the last named subjects they did not differ materially in time or content from those of Dr. Woodrow and Mr. Fullerton. At the end of the article the general results of all subjects are given.

We may first give a few tabulated results of the different experiments with the list of stimuli or ideas that came into consciousness in the intervals between observations of the stimulus in question. Relatively few are chosen but they will serve to give an idea of the way the experiments were carried on, and of the particular results.

In certain of the experiments some continuous stimulus was applied to determine what effect a distraction would have on the results. This is noted in the tables by the words after the + sign.

Tabulation of Results

Record I[1]

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*S*	Point in a picture	.2″	4.9″	11	2.7″	3.″	.98″
2	*S*	Point in a picture	.19	3.3	17	2.22	2.3	.656
3	*S*	Stone in picture	2.8	11.4	10	6.13	5.77	3.3

Record I.—Experiment 1.—Three of the disturbing elements were noises from without, twice a door slammed, and the rest were ideas concerning this experiment.

Experiment 2.—Two of the disturbing elements were noises from without, two were from the motor, twice the eyes moved to other parts of the picture, and the rest were ideas concerning the picture. The picture was a woodland scene with birds in it. The ideas were mostly concerning the birds.

Experiment 3.—The picture used in this experiment was that of an organ-grinder. One of the disturbing elements was a noise from without, one was the motor, one was a fly that lit upon the picture, and the rest were thoughts of the organ-grinder.

Record II

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*F*	Point in a picture	.2″	4.3″	31	1.23″	.8″	.69″
2	*F*	Point in a picture	.15	2.	39	.96	.8	.54
3	*F*	Point in a picture	.1	2.9	20	1.58	1.8	.76

[1] Below are given the meanings of the various abbreviations used: Exp., the number of the experiment; Sub., the subject; Sh., the shortest duration of attention and Lg., the longest; No., the number of trials in each experiment; Aver., Med., and M. V. mean, respectively, average, median, and mean variation; ″, seconds.

Record II.—Experiment 1.—As far as the subject could remember all of the disturbing elements were thoughts concerning his own experiments.

Experiment 2.—Two of the disturbing elements were noises from the other room, three were from talking outside, one was a cutaneous sensation (his finger on the key), and the rest were ideas. He had thoughts of going home, of a trip down town, of a picture of the new chemical building, of his friend and his telephone, etc.

Experiment 3.—Almost all of the distractions were disturbances from without.

RECORD III

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in a picture	.1″	7.7″	27	.947″	.6″	.931″
2	*W*	Point in a picture	.1	3.1	34	.98	.7	.654
3	*W*	Point in a picture	.01	2.3	22	.495	.4	.31
4	*W*	Point in a picture	.05	4.6	42	.767	.2	.76

Record III.—Experiment 1.—Two of the disturbing elements were his own breathing, two or three were noises from without, three were voluntary movements, two were flies buzzing, a number were shifts to other parts of the picture, and the rest were ideas concerning the experiment. The subject said that he had a tendency to neglect the point to examine what was going on in his mind.

Experiment 2.—The first disturbance was some one pounding, then the subject became conscious of the recording key, and the rest were due to a fluctuation from the point to the key.

Experiment 3.—Two or three of the distractions were due to organic sensations; two or three to noises; and the rest were thoughts concerning Dr. Shepard, his friend, and the picture until his foot went to sleep; then he stopped recording.

Experiment 4.—As reported by the subject, all of the disturbances were due to thoughts. Some of them were connected with the picture, but the most of them were memories of a visit that he had had that summer.

RECORD IV

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in a picture	2.6″	3.6″	4	3.18″	3.25″	.35″
2	*W*	Point in a picture	.6	5.3	6	2.5	2.05	1.5
3	*W*	Point in a picture	1.2	2.8	3	1.86	1.5	.65
4	*W*	Point in a picture	.15	2.6	7	1.18	.5	.84
5	*W*	Point in a picture	.6	4.	9	1.7	1.5	.7
6	*W*	Point in a picture	.2	2.9	7	1.18	.9	.6
7	*W*	Two lines in a picture	.01	5.65	12	1.83	.7	1.47
8	*W*	Point in a picture	.4	1.9	7	1.26	1.1	.51
9	*W*	Point in a picture	.01	4.3	71	.553	.3	.496
10	*W*	Point in a picture	.01	5.4	81	.57	.2	.49

Record IV.—Experiment 1.—The following are the disturbing elements in order of their appearance: the motor, idea of[1] . . . , breathing, and a sensation in nose.

Experiment 2.—In order of their appearance the first disturbing element was an idea of a man, the second was an idea of a bird, the third was a pain in the head, the fourth the subject noticed the recording key, and the rest he forgot.

Experiment 3.—The picture used in this experiment had a religious theme. The disturbances appeared as follows: The first two were ideas of Christ, the third was the idea of a figure two, and last he forgot.

Experiment 4.—The first distracting stimulus was the idea of a tulip, the next of a spade, for the next four the attention was drawn to the recording key, and the last was the idea that "I had better cut this out."

Experiment 5.—For the first two distracting stimuli the attention was drawn to the recording key, and the rest of them were organic sensations; lump in the throat, headache, etc.

Experiment 6.—The following are the distracting elements in their order: an idea of a girl's hand, her name, an organic sensation, and the last four he began to think that this is the third, fourth, fifth, and sixth.

Experiment 7.—In this experiment the subject noticed the key three times, his breathing once, and the rest were ideas, such as "I can keep these two lines in mind," a letter H, two lines, and the rest were about a sick friend.

Experiment 8.—I might say that the picture used for record

[1] Where blank spaces are left the introspections are too personal to permit of publication.

four was an animal picture containing all sorts of animals. The disturbing elements were all ideas as follows: lion, ass, dot, girl, tough, girl, ——.

Experiment 9.—Most of the disturbances in this experiment were explorations of other parts of the picture, a number of times he attended to the key; then he became conscious of what he was doing, the rest were divided between some one talking and sensations from the wrist.

Experiment 10.—The attention was drawn by a fly crawling on the picture and flying around, by some one walking, and the rest by ideas concerning a conversation with Dr. Shepard and other things which were personal.

RECORD XXII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in picture plus weak current	.01″	8.6″	38	1.98″	1.5″	1.22″
2	*W*	Same as 1 plus strong current	.1	2.9	80	.66	.3	.486
3	*W*	Same as 1 plus very strong current	.15	5.4	12	2.33	2.2	1.56
4	*W*	Same as No. 3	.01	4.5	27	1.68	1.3	1.05
5	*W*	Same as No. 3	.4	8.4	14	2.75	1.2	1.96
6	*W*	Point in picture plus very strong current	.2	8.7	26	2.55	2.1	1.44

Record XXII.—Experiment 1.—The picture used for this record was an animal picture containing many different kinds of animals. The subject noticed the current twice, contrast bands once, and the rest were ideas. He thought of a moose, a water hole, an eel, a fish, a lake at home, etc.

Experiment 2.—The attention was drawn mostly to the current, although there were many ideas.

Experiment 3.—These were all ideas concerning the animals.

Experiment 4.—The subject noticed the current twice, the recording key three times, the point shifted five or six times, and the rest were memories too personal to print.

Experiment 5.—The most of the disturbances were due to the current.

Experiment 6.—The current was noticed a number of times; this suggested a massage, which, in turn, suggested a barber shop in New York City; the subject noticed his light once and he swallowed once.

RECORD XXIII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*W*	Point in picture	.1″	5.6″	24	2.16″	1.6″	1.27″
2	*W*	Point on white paper	.02	5.7	26	2.19	2.	1.2
3	*W*	Point in a picture	.3	8.05	21	3.06	2.1	1.84
4	*W*	4-volt light in dark room	.1	6.7	54	1.13	.4	.79
5	*W*	Image a dot	.2	8.4	8	5.34	6.	2.3

Record XXIII.—Experiment 1.—The subject at first noticed his pulse and breathing two or three times, saw some lines near to the point, and the rest were ideas. He thought of ——, of ice, of the girl working with Mr. Carpenter, of his position for next year, of a glass of beer, and lastly "What a funny line of ideas."

Experiment 2.—With the following exceptions the disturbances were all memories which are personal. He noticed his breathing twice, his recording key twice, two or three shadows, and a contrast band.

Experiment 3.—The disturbances in this experiment were all personal memories of New York City.

Experiment 4.—All of these disturbances were memories, of which the following are a few: beer, champagne, Prof. Woodworth, Barnard Psychological Laboratory, a girl that used to cry when put into the dark room, etc.

Experiment 5.—The most of the time the subject was wondering if he had the image or not. The rest, with one exception, were personal memories.

RECORD XXXIII

Exp.	Sub.	Stimulus	Sh.	Lg.	No.	Aver.	Med.	M.V.
1	*F*	Dot on white paper	1.7″	2.9″	5	2.38″	2.4″	.34″
2	*F*	Point pressed on finger	1.5	4.5	3	3.	3.	1.00
3	*F*	Point pressed on finger	1.8	6.7	5	3.98	3.8	1.02
4	*F*	Image a dot	2.	3.8	6	3.16	3.6	.53
5	*F*	Dot on white paper	3.	3.6	5	3.26	3.2	.18
6	*F*	Image a dot	1.5	4.15	8	2.78	2.7	.55

Record XXXIII.—For the next six experiments the distracting elements are given in order of their appearance.

Experiment 1.—(1) The subject thought of a lead pencil, (2) saw the shadow of his hand, (3) became aware of the key, (4) forgot, (5) saw some contrast bands.

Experiment 2.—(1) He received a sensation from his leg, (2) forgot, (3) noticed his bowels roll.

Experiment 3.—(1) The subject thought of a crooked nail, (2) he had a burning sensation on his finger, (3) he imagined a nail going through a paper, the last two he forgot.

Experiment 4.—All of the disturbances were due to the image changing shape except one, for this one he imagined a cone on legs turning around.

Experiment 5.—(1) The subject's attention shifted to another part of the picture, (2) he thought of the cone in the last experiment, (3) he thought of going to Washington, (4) he saw a contrast band, (5) this one he forgot.

Experiment 6.—(1) The image got awfully big, (2) it got large again and began to roll, (3) the dot got square, (4) he forgot this one, (5) a white ring formed around the dot, (6) an idea about the dot, (7) it turned square again, (8) this one he forgot.

A great variety of stimuli was used so as to examine into the different fields of consciousness, *e. g.*, visual stimuli, as some particular point in a picture or a dot on a blank piece of paper, etc.; auditory stimuli, as a hissing or a buzzing noise; tactual stimuli, as a pressure varying from very weak to very strong, or an electric current (a great deal of stress was put on these experiments and the results were the same as for the other experiments); imaginary stimuli, as to keep in mind (with the eyes closed) some single thing as a point that they had seen, a kernel of corn, etc. These last aroused a great deal of interest among the subjects. It was impossible to keep the stimulus still. It would get large, then small, then turn over end for end, fade out and finally disappear altogether to return in a few seconds to go through its contortions again. The disturbing elements, or the things that came in to take the place of the stimulus attended to, were as a rule ideational. Sometimes the respiration, a slight headache, a noise, a fly, or some organic sensation (especially when the subject was new at the work) would take the place of the stimulus; but as soon as they had gotten used to the work the larger number of the disturbing factors were thought processes. Many times the

disturbing elements were suggested directly by the stimulus and at other times they seemed to have no relation, *e. g.*, in one experiment a point in a picture suggested to a subject a kernel of corn, this led to a past experience when the end board of his wagon fell out and let the back part of a load of corn on to the ground, this in turn suggested certain trouble that he had had with the people of whom he got the corn, and then other instances that happened while he was working on a ranch in Colorado came to him. Then, sometimes, the subject would wander off entirely from the experiment to his studies, to the dance the night before or to a banquet that he was going to attend the coming night, etc., yet they all served to distract the attention.

Secondly, as the subject became more acquainted with the experiments, the situation and his surroundings, and became more introspective, his attention period got shorter. A new subject lets many things come into consciousness and get out again without recording them. The stimulus might be gone for some time before he realized that it had gone; but as he became more expert there were fewer mistakes. These errors, however, would not shorten the duration of attention, but would lengthen it.

Thirdly, a complex stimulus tends to lengthen the attention period, see record No. 1, Exp. No. 3. The subjects were able to wander around within or about the stimulus itself, *e. g.*, if a complex stimulus as a house was used the subject could do a great deal of thinking in and about the house as to its shape, size, color, material, cost, etc., giving the mind a chance to fluctuate and yet not get away from the stimulus. This, however, is not holding the attention to one thing, but allowing it to shift to many.

Fourthly, suggestions work very differently with different subjects as can be seen from the tabulation of the introspections. On one subject in particular, Dr. Woodrow, it seemed to have little or no effect, while with Mr. Fullerton it played quite a part, *e. g.*, if certain things were suggested to be observed in the experiment they would often appear again to the latter, while they seldom ever came to the former.

Fifthly, a distracting stimulus as a bell, a buzzer, an electric current, etc., did not interfere with the attention but aided it if anything (see the results for Mr. Fullerton with the dot and the bell and the buzzer—Table I.). They might at first hinder, but soon the subject became accustomed to them and instead of interfering they shut out a great many intermittent, disturbing stimuli as noises and made a more ideal condition for attention, thus in some cases, tending to increase its period, or else, to do away with noises and to increase the number of ideational elements.

Lastly, a very interesting thing, to the author at least, was the fact that in the introspections the disturbing elements forgotten were, almost without exception, the last ones.

The differences in opinion as they appear in the literature of attention, are due, undoubtedly, not so much to a difference in knowledge of facts, as to a difference in point of view. One is talking about the focal point of consciousness or how long *one* thing can be kept in consciousness, while the other is talking about a general consciousness or how long we may be conscious *about* a thing. It is quite a different problem to ask how long one can keep an isolated thing in consciousness, as *e. g.*, to image a mere point or to keep any single image in mind, from asking how long one can think *about* a thing. In the latter case we might be attentive in and about a thing for days in our waking hours, but this is not holding attention to a single thing but to thousands. To keep the attention on one thing for any length of time is almost an impossibility. Consciousness in its very nature (see Titchener's 'Text-book in Psychology,' pages 15 and 16) is a stream, a continual flux, and cannot be held still to a single particular, but as soon as one thing enters another follows according to the law of association. It is also very important to distinguish between attending to an object continuously, and being morally certain it has been continuously present.

This continual flux of attention might be, and properly, called the attention wave. What has been called the attention wave, heretofore, are phenomena due or related, undoubtedly, to the vaso-motor or Traube-Hering wave or to similar physio-

logical processes. The change is altogether too slow to be called the attention wave. The periodic coming and going, *e. g.*, of a faint light in a dark-room, or any minimum stimulus, is very closely related to the rhythmic changes of the body determined perhaps by a center in the medulla. It is quite a different phenomenon from keeping attention fixed upon some single supraliminal stimulus.

The average duration of attention is in the neighborhood of two seconds. This conclusion is drawn from the results of work covering sixty laboratory periods for sixty days, during which time 441 experiments were performed that gave a total of 11,816 values. From these the following general averages may be given for the different observers:

TABLE I

FOR MR. FULLERTON

For Visual Stimuli

	Aver. Aver.	Aver. Med.	Aver. M.V.
Point in picture	2.037	2.099	.640
Point in picture plus weak current	2.411	2.390	.770
Point in picture plus moderate current	2.013	1.800	.743
Point in picture plus strong current	1.580	1.550	.620
Point in picture plus a suggestion	2.130	1.958	.905
Point in picture plus buzzer or bell	2.066	1.881	.719
Dot on white paper	1.940	1.940	.623
Dot on white paper plus moderate current	2.130	1.958	.905
Dot on white paper plus strong current	1.160	1.000	.500
Dot on white paper plus a suggestion	1.775	1.500	.240
Dot on white paper plus buzzer or bell	2.330	2.126	.886
A 4-volt light	2.315	2.225	.520
For Cutaneous Stimuli			
Point pressed on finger	2.480	2.450	.747
Point pressed on finger plus buzzer	1.620	1.500	.470
A moderate current on arm	1.170	.600	.780
For Image as Stimuli			
Image of a single thing	2.102	1.983	.621
Image of a single thing plus suggestion	2.120	1.830	.660
Image of a single thing plus buzzer or bell	2.220	2.200	.620

TABLE II

FOR DR. WOODROW

For Visual Stimuli

	Aver. Aver.	Aver. Med.	Aver. M.V
Point in picture	1.888	1.410	.966
Point in picture plus weak current	2.310	2.050	1.130
Point in picture plus moderate current	1.898	1.070	.989
Point in picture plus strong current	2.042	1.320	1.330
Point in picture plus a suggestion	1.540	1.230	.671
Point in picture plus buzzer or bell	1.850	1.950	.910
Dot on white paper	1.700	1.390	.900
Dot on white paper plus weak current	1.840	1.100	1.360
Dot on white paper plus moderate current	1.735	1.113	1.128
Dot on white paper plus strong current	2.020	1.350	1.320
Dot on white paper plus a suggestion	1.440	1.450	.520
Dot on white paper plus buzzer or bell	1.886	.960	1.272
Dot on white paper plus moderate current and buzzer	1.930	1.350	.830
A faint light (four volts)	1.345	.820	.710
For Auditory Stimuli			
The hum of the motor	0	1.700	1.380
The hissing of the radiator	0	1.820	.800
For Cutaneous Stimuli			
Point pressed on finger	2.417	1.825	1.372
Point pressed on finger plus buzzer	1.600	1.000	1.503
Pain (sharp point pressed on finger)	.360	.300	.760
A moderate current on arm	1.440	1.000	1.090
For Image as Stimuli			
Image of a single thing	2.620	2.216	1.380

TABLE III

The following are the average results for the different senses for Mr. Fullerton.

	Aver.	Aver. Med.	Aver. M.V.
For visual sensations	1.990	1.869	.672
For cutaneous sensations	1.76—	1.520	.665
For memory images	2.144	2.004	.634

TABLE IV

The following are the average results for the different senses for Dr. Woodrow:

	Aver.	Aver. Med.	Aver M.V.
For visual sensations	1.816	1.326	1.002
For auditory sensations	2.070	1.760	1.090
For cutaneous sensations	1.454	1.031	1.181
For memory images	2.620	2.216	1.380

TABLE V

The following are the total averages for the different subjects:

	Aver.	Aver. Med.	Aver. M.V.
For Mr. Fullerton	1.9643	1.7976	.637
For Dr. Woodrow	1.9900	1.5832	1.164
For Dr. Shepard	2.5322	1.6232	.862
For Mr. Crane	1.9023	1.6621	.730
For Mr. Cook	1.7613	1.7536	.891
For Mr. Winter	1.9832	1.7301	.932

In way of summary, the duration of attention, according to these experiments, is very short; the average duration of attention is but a little over two seconds, the average median of attention is 1.69 seconds, and the average mean variation (average deviation) is less than .9 of a second; the disturbing elements were in the majority ideational, or memories; the average duration of attention is much longer with a complex stimulus than with a simple one; suggestions function as disturbing elements with some subjects while with others it does not; and a distracting stimulus as a bell or a buzzer if continuous does not ordinarily interfere with the attention.

ADDENDUM

After this manuscript had been made ready for the press I received the results of the work of Mr. Ernest Work who had carried on a series of experiments with a view to determining the duration of attention before I began mine. As I cannot very well embody them in my own work I shall give them in this addendum. Mr. Work carried on these experiments while in the University of Michigan but gave them up and his results when he left.

Mr. Work experimented with fifteen different subjects but he, like myself, worked with some of them only a short time while he worked a whole semester with others. Two or three of his subjects had had experience in introspecting and the rest had had none.

Souvenir postal cards were used for all of these experiments. The tests were all visual. For the first set of experiments, eight subjects were tested. "The subject directed his attention

upon some particular point in the picture to the exclusion of all else." The following are the average results:

The average number of trials in each experiment..... 10.17
The average duration of attention in all of the experiments..... 2.46″
The average M.V. of attention in all of the experiments..... .682″

In another set of experiments Mr. Work tried to measure the duration of attention without trying to keep the attention on any definite thing. "We started with some one point or stimulus, then let the attention go at will to whatever attracted it at the moment." Six subjects were used for these experiments. The following is a summary of his results:

The average number of trials in each experiment..... 11.70
The average duration of attention in all of the experiments..... 2.55″
The average M.V. of attention in all of the experiments..... .613″

"It will be seen by the above that it seems to make very little difference in the time of attention period between the trials when the object was to get back to the same spot and the trials when the attention was allowed to go at will. . . . It would seem from this that effort in attention has little or nothing to do with the length of time that attention may be kept upon one thing."

"Another point of interest got from a number of subjects . . . is that the change from one center of attention to another is not abrupt and all at once, but gradual. The one period fades out—shades off—into the next."

The next tests were to determine the effect of distracting stimuli on attention. Three subjects were used for these experiments. The subjects were first tested without, then with a current with the following results:

The average number of trials in each experiment (without current)..... 9.72
The average duration of attention in all of the experiments..... 2.83″
The average M.V. of attention in all of the experiments..... .564″

Experiments with weak current:

The average number of trials in each experiment..... 10.75
The average duration of attention in all of the experiments..... 3.37″
The average M.V. of attention in all of the experiments..... .517″

Experiments with strong current:

The average number of trials in each experiment..... 15.97
The average duration of attention in all of the experiments..... 2.10″
The average M.V. of attention in all of the experiments..... .856″

"It will be seen from these experiments that a slight stimulus aids attention to an object; *i. e.*, increases the period of attention, but when a certain intensity is reached the new stimulus ceases to aid and detracts from the attention time. Individuals, however, are not all the same in this, nor is the same person the same on different days and under different conditions. It aids sometimes and detracts at others. My first subject always did better without the stimulus. All the others, however, did better with a slight stimulus."

These results are of interest to the writer as they are so similar to his own. I might say that neither of us knew of the other's work. I knew nothing of Mr. Work's methods, the points that he was investigating, or anything, other than that he had been doing work along this line. So these results were worked out entirely independently of one another with a surprising similarity in the outcome.

M. L. B.

THE EFFECT OF SIZE OF ADVERTISEMENTS AND FREQUENCY OF THEIR PRESENTATION[1]

BY EDWARD K. STRONG, JR.

Columbia University

The problem, the solution of which we hoped to reach through this experiment, is as follows: Which should preferably be run, (1) one 1-page advertisement in four months, or (2) two ½-page advertisements two months apart, or (3) four ¼-page advertisements, one each month? In other words, what is the truth with regard to the statement that "small space in many magazines is better than large space in few magazines."

The practical applications of this study have already been presented to advertising men[2] and we shall not concern ourselves with them now. But there are two points of interest to the psychologist which we shall consider here. These two points are: (1) how does an increase in the size of an advertisement affect the permanency of impression made upon the reader? and (2) how does continued repetition of a firm's advertisement affect this permanency of impression? Stating the problem in concrete language we have (1) what is the efficiency of a ¼-page advertisement as compared with a 1-page advertisement and (2) what is the efficiency measured in the permanency of impression of a firm who advertises once in four months and a firm who advertises four times in the four months? These two problems are purely psychological in nature. They are problems that the psychologist ought to be able to answer. But they are problems which the psy-

[1] This report presents the psychological aspects of a study made for the Association of National Advertising Managers whose research fellowship I hold.

I wish to express here my indebtedness to Prof. R. S. Woodworth and Dr. H. L. Hollingworth and to their assistants, Mr. G. F. Willianson and Miss Mabel Barrett, for the help they have afforded me in securing subjects for this experiment.

[2] 'Size and Frequency in Advertising,' Association of National Advertising Managers, Research Bulletin No. 4, April 1913.

chologist has not been able to answer except on the basis of theories which he would not be at all sure would apply in this particular case.

The Experiment

The experiment was as follows: After a great deal of care the advertisements of 144 different firms were selected for use. One third of these firms were using 1-page advertisements, one third were using ½-page advertisements, and the remaining third were using ¼-page advertisements. Four advertisements were used from 12 firms from each of the above three groups, two advertisements were used from 12 other firms from each of the three groups, and one advertisement was used from the remaining 24 firms in the three groups. In this way 288 different advertisements were employed in all.

These 288 advertisements were divided into four sets corresponding to four monthly issues of a standard magazine. In each of these four sets there was one advertisement from each of the 36 firms of which 4 advertisements were being used. In other words, these 36 firms advertised in each one of the four dummy magazines. The firms of which 2 advertisements were used from each, were represented in alternate sets. In other words these firms advertised every other month in the dummy magazines. And in the same way the firms of which only one advertisement was used advertised once in the four issues of the dummy magazines.

We have then, to repeat, the following situation:—

12 firms using full-pages and advertising 4 times.
12 firms using full-pages and advertising 2 times.
24 firms using full-pages and advertising 1 time.
12 firms using half-pages and advertising 4 times.
12 firms using half-pages and advertising 2 times.
24 firms using half-pages and advertising 1 time.
12 firms using quarter-pages and advertising 4 times.
12 firms using quarter-pages and advertising 2 times.
24 firms using quarter-pages and advertising 1 time.

Two ½-page advertisements were pasted on a sheet the size of a full-page and four ¼-page advertisements were treated in the same way. The 1-page advertisements were similarly treated. Position of the half and quarter page advertisements

on the sheets was determined by chance but care was taken that advertisements from the same firms would not appear twice together on any page. In presenting the advertisements to the persons tested, the order of the sheets was constantly changed by shuffling them. In this way no advertisement had any advantage over any other advertisement through the position of that advertisement in the set.

Two different methods of presenting the advertisements were followed. With half of the subjects the sheets were shown at a uniform rate of one per second. With the other half the sheets were given to the subjects and they were instructed to look them through at their leisure. It was suggested that they look them through in the same way that they would turn the pages of an advertising section of any magazine looking at what interested them and ignoring the rest. In each case they were timed by a stop-watch. The first method was employed to satisfy the experimenter, the second to satisfy advertising men. But as both methods give us proportionately the same results, it is not necessary to discuss their relative merits.

Sixteen men and fifteen women were tested by the first method and nine men and fourteen women by the second method. Due to sickness, dropping college, etc., only ten men and eleven women of the first group and six men and twelve women in the second group completed the work. Although but 39 persons in all were tested, the uniformity of the results and the low probable errors are evidence that the results of the experiment are very reliable.

The four sets of advertisements were shown to the subjects a month apart. One month later they were tested as to their remembrance of what had been shown them. In this test they were shown the last advertisement shown them from each firm together with an equal number of wrong advertisements. They were instructed to pick out all the advertisements which they had seen previously in the test. If they were sure any advertisement had been seen before they were instructed to pick it out. Moreover, if they were not sure that the advertisement before them was the one they had seen, but they were sure it was the same *firm*, that was sufficient.

A word might be said as to the real value of a recognition test, as used here, for determining the permanent impression of a series of advertisements. Psychologically the situation which most advertising is aimed to meet is first and primarily, the development of a very strong associative bond between *a need for a commodity* and *a trade-name*, as *need of soap* and *Ivory*, and second, the development of a very favorable attitude toward that trade-name. There is no desire to develop associative-bonds between a certain magazine and the advertisements displayed in it. Inherently then a recall test is of little or no value. In fact, the writer agrees with some advertising writers when they state that an advertisement remembered for its own sake (as it must be in a recall test) is very much of a failure. For the attention of the reader has been directed not to connecting a need with a remedy but to praise of an advertisement as an advertisement. The recognition test, on the other hand, tests what the reader paid attention to originally when he looked through the magazine. Nearly all will agree that advertisements that were *not* paid attention to in the magazine will not be recognized later,—except on mistaken grounds. And as my work has already shown that under ordinary conditions very few advertisements are selected which have not been shown before, we are not bothered in this test particularly by such mistaken recognitions. The only criticism of the recognition test lies in the fact that a reader *might* pay equal attention to three advertisements, for example, and yet not recognize one of them later. This seems impossible to the writer but after all we do not know the facts. The recognition test then determines which advertisements were noticed originally and which were not. And when degrees of certainty of the recognitions are asked for then we obtain some light also as to the strength of the impression.[1]

(The Treffer- und Zeitmethode might be employed in this connection in this way. After the advertisements have been displayed, present a list of the commodities that have been advertised and determine the trade-name given and the time

[1] Further consideration of this point is given in 'Psychological Methods as Applied to Advertising,' *Jour. of Educ. Psychol.*, *4*, 1913, 393–404.

of response. But the weakness of such a procedure lies in the difficulty of determining before the experiment commences the strength of the associations then in existence between commodity and trade-name and also the increase of associative strength that takes place between exposure and the test from other sources than those included in the test itself. This second difficulty is probably insuperable when an experiment continues for four months, for the advertising seen on every hand can not be excluded from the subjects.)

Before turning to the results of this experiment it is well to consider some of the precautions that were observed in the work.

1. Only advertisements from the twelve issues of the 1911 *Everybody's* magazine were used. As the advertisements were old ones, they were not likely to be seen outside of the test.

2. On the basis of the experimenter's and his wife's judgments all advertisements were excluded of firms which the average person thinks of as being always present in the standard magazine, such as Ivory Soap. All clues were thus eliminated as to what advertisements ought to have been in the four sets.

3. But one commodity of any firm was used in the test. Hence if the firm name was remembered full credit would be given to the advertisement which displayed it.

4. No two firms using full-page advertisements advertised the same commodity. The same was true of the firms using half-page and quarter-page advertisements. There were a few instances where the same commodity but of different firms was advertised in different sized advertisements. This was necessary because of a lack of the requisite advertisements. The error arising from these few cases, if there was any, would be too small to affect the results in an appreciable manner.

5. The individuals tested were allowed to assign a grade of 100 per cent., 75 per cent., or 25 per cent. validity to the recognitions of each advertisement which they selected as having been shown them before. In this way those advertisements which had made the most impression on each individual could be considered separately from those advertisements which were remembered but had made slight impression.

6. Pure guessing in the tests on the part of the subjects was eliminated through an elaborate system of grading the data.[1] For example, a person who picked out an equal number of right and wrong advertisements was not considered since chance could account for such results. In a similar way the more accurate the work of an individual the more it counted toward the final results.

The Results of the Experiment

1. *The Total Per Cent. of Firms Remembered.*—Table I. presents the results on this point. The per cents. given in this table seem very low. But when it is remembered that the advertisements were shown in four sets separated by an interval of a month and that the test followed a month later one should not be surprised at the results. In fact they are higher, especially in the case of the second method, than the experimenter expected.

Table I

Showing the Per Cent. of Firms Remembered per Reader

	Method of Presentation	Per Cent. of Firms Remembered Per Reader
1st method.	Pages shown at rate of 1 per sec.	2.1 per cent. (P. E. *0.4*)
2d method.	Pages shown at leisure of reader.	6.3 per cent. (P. E. *0.8*)

The individuals who were allowed to look at the advertisements at their leisure remembered 3 times as many as the individuals who were allowed but one second per page. The former group average 219 seconds per set while the latter had about 63 seconds. The former group spent 3½ times as much time in looking at the advertisements and remembered 3 times as much. This result agrees very well with Ebbinghaus's results where he found a direct proportion between the number of repetitions (or time spent on a series) and the saving in learning the series 24 hours later.[2]

There is a correlation of +.65 between the time spent by the subjects of the second method in looking at the advertise-

[1] For a general description of the method employed here, see the writer's article, 'The Effect of Length of Series upon Recognition Memory,' Psych. Rev., November, 1912, **19**, 447–462.

[2] H. Ebbinghaus, 'Das Gedachtnis,' 1885, Chap. VI.

ments and the per cent. recognized. This correlation would be raised to +.80 if the records of one individual were omitted. In a much more difficult test made on these same subjects immediately after the one described here a correlation of +.82 is obtained between time spent in looking at the advertisements and the per cent. recognized. And if the same individual is again omitted the correlation is raised to +.88. It seems very surprising to the writer that there should be such a high correlation between these two factors, since there is a very great variation between the records of subjects when the advertisements are presented at the rate of one page per second.

2. *The Per Cent. of Firms Remembered for Each of the Nine Combinations of Space and Frequency.*—Table II. gives the percentage of firms remembered per reader for each of the nine combinations of space and frequency. In order to get clearly before us the meaning of these figures we shall consider first the factor of *size of space* and then the factor of *frequency* and after that return to this table for a general view taking into consideration both of the factors.

TABLE II

SHOWING THE PER CENT. OF FIRMS REMEMBERED PER READER FOR EACH OF THE NINE COMBINATIONS OF SPACE AND FREQUENCY[1]

1. *When the Pages Were Shown at the Rate of One per Second*

Frequency	Size of Advertisements			Probable Errors of Averages		
	¼-page	½-page	1-page	¼-page	½-page	1-page
Once	1.7	2.3	3.6	*0.2*	*0.3*	*0.4*
Twice	2.2	3.1	4.6	*0.3*	*0.6*	*0.7*
4 times	3.0	3.9	6.0	*0.6*	*0.7*	*0.8*

[1] The figures in this table and those of Table I. do not correspond exactly. In Table I. chance selections which were correct were cancelled in the total score by corresponding chance selections which were incorrect. In Table II. this could not be done in such a way that the writer could feel sure the process had not influenced the results he was endeavoring to obtain. Table II. contains then results certainly to be attributed to genuine memory plus a certain amount undoubtedly due to chance. The latter factor is larger in the results from the first method of presenting the advertisements than from the second method. The writer does not feel that the presence of these chance results can affect the general results obtained in the experiment, while their elimination by the only method known to him might have done so.

2. *When the Pages Were Viewed at the Reader's Leisure*

Frequency	Size of Advertisements			Probable Errors of Averages		
	¼-page	½-page	1-page	¼-page	½-page	1-page
Once	3.8	5.9	8.7	*0.5*	*0.7*	*0.9*
Twice	4.4	7.0	10.5	*0.8*	*1.0*	*1.3*
4 times	6.5	8.2	13.0	*1.0*	*0.8*	*1.4*

3. *Value of Space.*—If now we take the per cents. in the two parts of Table II., and express them in terms of ratios, letting the value of the ¼-page advertisement each time represent 1.00, we have the proportions shown in Table III.

TABLE III

SHOWING THE VALUE OF ¼-PAGE, ½-PAGE, AND 1-PAGE SPACE IN TERMS OF RATIOS OF ¼-PAGE SPACE

Frequency	Rate of Exposure	¼-page	½-page	1-page
Firms advertising once	1 per second	1.00	1.33	2.12
	Leisure of reader	1.00	1.55	2.29
Firms advertising twice	1 per second	1.00	1.41	2.09
	Leisure of reader	1.00	1.59	2.39
Firms advertising 4 times	1 per second	1.00	1.30	2.00
	Leisure of reader	1.00	1.26	2.00
Average of all 6 ratios		1.00	1.41	2.15

These figures indicate that a ½-page space is not twice as efficient as a ¼-page space but only 41 per cent. more efficient. They also indicate that a 1-page space is only twice as efficient as a ¼-page space although occupying four times the area. Moreover, this relationship between the three sized advertisements considered here holds equally well whether a firm runs one advertisement, two advertisements, or four advertisements. In each case the ratios of efficiency remain approximately 1.00 : 1.41 : 2.15.

In this connection let us consider some other evidence bearing on this point.

(*a*) In our experiment[1] with *Everybody's* magazine upon

[1] The four experiments given here were by-products to various experiments and the particular facts stated here have not been published before. The ratios given here can only be considered as approximations to the true relationship due to the presence of varying conflicting factors.

36 women who did not know that they were going to be tested (eliminating full-page advertisements in preferred position) the ratios of efficiency for ¼-page, ½-page, and 1-page advertisements were: 1.00 : 1.11 : 1.13.

(*b*) In a similar experiment with the *National Geographic Magazine* with 56 persons who did not know they were going to be tested the ratios were: 1.00 : 2.39 : 3.65.

(*c*) And upon 60 persons who knew they were going to be tested the ratios were: 1.00 : 1.53 : 2.34.

(*d*) In another experiment where 2 ½-page advertisements together were compared with their efficiency when appearing one at a time on a page, and similarly with the ¼-page advertisements. The ratios obtained in this case were: 1.00: 1.60 : 2.51.

The averages of these four relationships are: 1.00 : 1.66 : 2.41.

Besides these experimental results, I quote this one from practice.

(*e*) The ratios between inquiries per advertisement from 10 ¼-page advertisements, 19 ½-page advertisements, and 22 1-page advertisements, as given by Shryer[1] are: 1.00 : 1.61 : 2.26.

All these ratios show that a 1-page advertisement is not twice as efficient as a ½-page advertisement nor four times so efficient as a ¼-page advertisement and (with one exception) that a ½-page advertisement is not twice so efficient as a ¼-page advertisement. On the other hand they all approximate much more closely to the ratio: 1.00 : 1.41 : 2.00 than to the ratio 1.00 : 2.00 : 4.00. In other words the efficiency of space increases approximately as the square root of the area and not directly as the area (1.00 : 1.41 : 2.00 is the same ratio as that of $\sqrt{1} : \sqrt{2} : \sqrt{4}$).

Scott's[2] deduction from experiments carried out by him give just exactly the opposite result. He states 'that the full-page advertisement was more than twice as effective as a half-page advertisement and a half-page was more than twice as effective as a quarter-page.' His results are based on experiments in

[1] W. A. Shryer, 'Analytical Advertising,' 1912, p. 190.

[2] W. D. Scott, 'The Psychology of Advertising,' 1908, p. 172.

which the reader afterwards recalled what he could of the advertisements he had seen. The writer has found this method very inadequate, as many persons, even when knowing they are going to be tested, can carefully look through a magazine's advertising sections and yet recall but one or two advertisements. Moreover, those that they do recall are generally the advertisements that are always to be found in the magazine, such as Ivory soap. Scott did not even guard against this tendency for he tells us that the Ivory soap advertisement was most often mentioned, the In-er-Seal advertisement was next and Pears' soap was third. In a recent letter to the writer Scott admits that "the better data from the full-page advertisements than from the smaller ones and more than would be expected because of the increase in size . . . may have been the result of previous education and not of the independent observation at the time of the experiment."

Scott also employed a recognition test. The results from that test indicate in one case that a full-page advertisement is not quite twice so effective as a half-page advertisement. Beyond this one contradiction to the results obtained by the recall-method his two experiments agree as to the general conclusion stated above. A number of precautions must be observed in such experiments, *e. g.*, (1) the elimination of preferred position, which only benefits full-page advertisements; (2) the elimination of well-known firms, which benefits full-page advertisements much more than smaller space advertisements; and (3) the weighing of the accuracy of the recognitions, else careless and cocksure individuals will influence the totals much more than careful and conservative individuals. Scott gave some attention to the factors of preferred position and of well-known firms but no attention to the weighing of the accuracy of recognitions. In the writer's opinion these factors, not completely guarded against by Scott, account for the difference between his results and those of this report.

We must conclude then that the value of space increases approximately as the square root of the increase in area and not directly with the increase in area. A ¼-page space is worth $^{7}/_{10}$ of a ½-page space and ½ of a full-page.

4. *Value of Frequency of Repetition When There is a Considerable Interval of Time between Each Presentation.*—Taking the per cents in Table II. and expressing them in terms of ratios, this time letting the value of one presentation represent 1.00, we have the proportions shown in Table IV.

TABLE IV

SHOWING THE VALUE OF ONE PRESENTATION, TWO PRESENTATIONS, AND FOUR PRESENTATIONS IN TERMS OF ONE PRESENTATION. (Based on Table II.)

Size of Ad.	Rate of Exposure	Number of Presentations		
		One	Two	Four
¼-page	1 page per second	1.00	1.29	1.76
	Leisure of reader	1.00	1.16	1.71
½-page	1 page per second	1.00	1.35	1.70
	Leisure of reader	1.00	1.19	1.39
1-page	1 page per second	1.00	1.28	1.67
	Leisure of reader	1.00	1.21	1.49
Average of the 6 ratios		1.00	1.25	1.62

It is evident from these ratios that two presentations add only one fourth more to what one presentation has accomplished and that four presentations add only two thirds more to what one presentation has accomplished. Off-hand one might have expected ratios of 1.00 : 2.00 : 4.00. That is, that two presentations would be twice as effective as one, and four presentations four times as effective as one. But we do not get this relationship at all. It is also evident that there is no appreciable difference between ¼-page, ½-page, or 1-page space as far as the relative increase of permanent impression is concerned due to repetition. In other words repetition increases the value of one presentation of the three sized advertisements in the same way, by adding one fourth more with a second presentation and two thirds more with three more presentations.

If the two methods of presenting the advertisements to the subjects are considered separately we have the following ratios as to the value of one, two, and four presentations:

Ads shown at rate of 1 page per second, 1.00 : 1.31 : 1.71.
Ads shown at the leisure of the reader, 1.00 : 1.19 : 1.53.

Apparently from these figures, the individual who looks at advertisements at his leisure pays relatively more attention to the first presentation of a firm's advertisements than to the succeeding presentations than does the individual who has but one second to peruse a page. But for the purposes of this experiment an average of the two sets of ratios is the safest measure of the relationship.

Conclusion.—In this case repeated presentations do not have proportionate efficiency. One, two, and four presentations do not have ratios of 1.00 : 2.00 : 4.00 but rather ratios of efficiency of 1.00 : 1.25 : 1.62. (Curiously these ratios are approximately equal to the cube roots of the number of presentations, *i. e.*, $\sqrt[3]{1} : \sqrt[3]{2} : \sqrt[3]{4}$ equals 1.00 : 1.26 : 1.59.)[1]

5. *Value of Space and Frequency Considered Together.*—We have seen already that the value of space does not increase proportionately with the increase in area but that when

¼-page space equals an efficiency of 1.00, then
½-page space equals an efficiency of 1.41, and
1-page space equals an efficiency of 2.15.

And we have seen that the values of successive repetitions a month apart of a firm's advertisements do not increase proportionately with the increase in presentations but that when

1 presentation equals an efficiency of 1.00, then
2 presentations equal an efficiency of 1.25, and
4 presentations equal an efficiency of 1.62.

TABLE V

SHOWING THE VALUE OF THE NINE COMBINATIONS OF ¼-PAGE, ½-PAGE, AND 1-PAGE SPACE PRESENTED ONCE, TWICE, AND FOUR TIMES.

Each value is stated in terms of the value of a ¼-page advertisement shown once. (Ratios obtained from averaging the two parts of Table II.)

Number of Presentations	Size of Advertisement		
	¼-page	½-page	1-page
1	1.00	1.45	2.20
2	1.22	1.83	2.73
4	1.73	2.22	3.47

[1] The ratios obtained here apply to *intervals of one month* between presentations. From data now being accumulated we find that shorter intervals, as one week, give ratios indicating a greater effect from two or four presentations than shown here.

Now combining the two factors we get the ratios shown in Table V. Here efficiency of each combination of size and frequency is stated in terms of the efficiency of a ¼-page advertisement shown once.

This table shows us very clearly that a ½-page advertisement shown once in four months is superior in the permanency of its effect to 2 ¼-page advertisements shown two months apart, for the relative values of the two are:

½ page shown once in 4 months equals 1.45
¼ page shown twice in 4 months equals 1.22

It shows us that a 1-page advertisement shown once in four months is superior in the permanency of its effect to 2 ½-page advertisements shown two months apart, or to 4 ¼-page advertisements shown once a month, for the relative values of the three are:

1 page shown once in 4 months equals 2.20
½ page shown twice in 4 months equals 1.83
¼ page shown 4 times in 4 months equals 1.73

And the table shows us that 2 1-page advertisements shown in 4 months are superior to 4 ½-page advertisements shown during the same interval, for the relative values of the two are:

1 page shown twice in 4 months equals 2.73
½ page shown 4 times in 4 months equals 2.22

It is very evident then that for the same total amount of space used during four months one obtains a greater permanency of impression by using in the same magazine large space and less often than by using small space and more frequently. It is very easy to see that this must be the case in this particular situation for permanency of impression increases approximately as the square-root of the space used but only as the cube-root of the number of presentations. Hence, to repeat, the same amount of space used in large advertisements seldom repeated must be more effective for permanent impression than when used in small advertisements more frequently repeated.

6. *Value of Space and Frequency Considered together when the Frequency Consists of Presentations Occurring within a Few Minutes of Each Other.*—The writer knows of no other experimental work where a study has been made of the value of

successive presentations in which the presentations were separated by a considerable interval of time. But there have been a number of studies where the effect of successive presentations has been studied in which the presentations occurred within a few minutes of each other. A word might be said here as to the effectiveness of such repetitions (1) as it has been found in advertising and (2) as found in other phases of mental response.

Based partly upon experiment and partly upon theory, the writer prophesied[1] that two presentations of a firm's ½-page advertisements run in the same magazine would have greater efficiency than a full-page advertisement. The results from one advertising firm, who tried the stunt, show a ratio of efficiency of 116 to 100 in favor of the 2 ½-page advertisements.

Münsterberg,[2] reports an experiment in which were determined the effectiveness of a full-page advertisement appearing once, a ½-page advertisement appearing twice, a ¼-page advertisement appearing four times, a ⅛-page advertisement appearing eight times, and a 1/12-page advertisement appearing twelve times;—all appearing in a set of 60 pages of advertising. His values based upon immediate impression are:

A 1 page advertisement shown once had a memory-value per reader of 0.33
½ page advertisement shown twice had a memory-value per reader of 0.30
¼ page advertisement shown 4 times had a memory-value per reader of 0.49
⅛ page advertisement shown 8 times had a memory-value per reader of 0.44
1/12 page advertisement shown 12 times had a memory-value per reader of 0.47

In regard to the assigned value of the 1-page advertisement shown once Münsterberg states that he was "inclined to believe that the ascent of the curve of the memory-value from the full-page to the fourth-page or eighth-page would have been still more continuous, if the whole-page advertisements had not naturally been such as are best known to the American reader. The whole-page announcement, therefore, had a certain natural advantage." This use of full-page advertisements of well-known firms undoubtedly raised the obtained value above its true value, just as we have found was the case with Scott's experiments.

[1] 'On the Amount of White Space Valuable for Attention or Isolation Value,' Association of National Advertising Managers, Research Bulletin, No. 1, October 16, 1912.

[2] H. Münsterberg, 'Psychology and Industrial Efficiency,' 1913, Chap. XX.

Smith's[1] experiment is the only other experimental study known to the writer that is directly applicable here in that the subjects were not expected to memorize the material before them but were requested simply to look at it. In that study the students read through different series of 10 nonsense syllables. They 'were directed not to try and learn as much as possible but simply to repeat, with all possible regularity what was presented to them.' The results were as follows:

After one reading of the series 22 per cent. could be reproduced.
After 3 readings of the series 25 per cent. could be reproduced.
After 6 readings of the series 28 per cent. could be reproduced.
After 9 readings of the series 34 per cent. could be reproduced.
After 12 readings of the series 39 per cent. could be reproduced.

Smith concludes that 'the first repetition is undoubtedly the best, *i. e.*, more is learned by it than by any other repetition, or in fact, by all the other (11) repetitions put together.'

In studies where the student was expected to try and learn as much as possible of a series of numbers, the results agree on the whole with the above. Thus, for example, Hawkins[2] found (averaging all his results) that after

1 reading 47 per cent. was recalled—in terms of a ratio 1.00
2 readings 42 per cent. were recalled—in terms of a ratio .90
3 readings 59 per cent. were recalled—in terms of a ratio 1.25

and Smedley[3] found that after

1 reading 47 per cent. was recalled—in terms of a ratio 1.00
2 readings 55 per cent. were recalled—in terms of a ratio 1.17
3 readings 59 per cent. were recalled—in terms of a ratio 1.25

Other experimenters, such as Pohlmann[4] and Henderson,[5] agree as to the same general relationship. But the latter finds in his study of prose passages that the longer study 'seems more effective in increasing the percentage of words and ideas

[1] W. G. Smith, 'The Place of Repetition in Memory,' PSYCHOL. REV., 3, 1896, pp. 21–31.

[2] C. J. Hawkins, 'Experiments on Memory Type,' PSYCHOL. REV., 4, 1897, 289–294.

[3] F. W. Smedley, 'Child Study in Chicago,' U. S. Bureau of Education, 1902, I. 1134.

[4] A Pohlmann, 'Experimentelle Beitrage zur Lehre vom Gedachtnis,' Berlin, 1906.

[5] E. N. Henderson, 'A Study of Memory for Connected Trains of Thought,' PSYCHOL. REV., Monog. Sup., No. 23, 1903.

retained' after some time. But he does not get anything like a direct ratio between the number of repetitions and the amount retained.

Ebbinghaus,[1] however, found in learning series of 16-syllable series that there was a direct relationship between the number of repetitions employed today and the number gained in relearning tomorrow. That is, if the series was repeated

		A gain Per Repetition of
0 times, it required	1,270 seconds to learn it 24 hrs. later.	
8 times, it required	1,167 seconds to learn it 24 hrs. later.	12.9 secs.
16 times, it required	1,078 seconds to learn it 24 hrs. later.	12.0 secs.
24 times, it required	975 seconds to learn it 24 hrs. later.	12.3 secs.
32 times, it required	863 seconds to learn it 24 hrs. later.	12.7 secs.
64 times, it required	454 seconds to learn it 24 hrs. later.	12.8 secs.

But if the series was relearned not as exactly studied the first day but as rearranged in this way: 1st syllable, 3d syllable, 5th syllable, . . . 15th syllable, 2d syllable, 4th syllable, . . . 16th syllable, then

		A gain Per Repetition of
0 repetitions required	1,270 seconds to learn it 24 hrs. later.	
16 repetitions required	1,170 seconds to learn it 24 hrs. later.	6¼ secs.
64 repetitions required	1,109 seconds to learn it 24 hrs. later.	2½ secs.

"Quadrupling the repetitions resulted here in increasing the saving only a little more than half as much again" (100 seconds as compared with 160 seconds). The results obtained in this last case are very similar in their general relationship to those obtained in our own results. The ratio of presentation is 1.00 : 4.00 while the efficiency is but 1.00 : 1.61. In our experiments when the ratio of presentations was 1.00 : 4.00 the efficiency was but 1.00 : 1.62.

The situation, as far as can be determined from experimental work now in existence to my knowledge is this. The effect of repeated presentations varies enormously under varying conditions. What the laws are we do not know. But it seems possible that the more difficult the situation the less proportionate are the results of repetition. For when the presentations are a month apart a single presentation of a 1-page advertisement is more efficient than four presentations of ¼-page advertisements, but when the presentations are separated by only

[1] H. Ebbinghaus, *op. cit.*

a few minutes then the reverse is the case. In other words, it seems possible that there is an optimum length of interval between successive presentations. When that interval is lengthened the effect from each presentation is weakened. But what that optimum interval is we do not know.[1]

Conclusion

1. An approximately direct relationship was found between length of exposure of advertisements and the number that could be correctly recognized on an average of 2½ months later.

2. The value of space in advertising as affecting permanent impressions increases approximately as the square-root of the increase in area and not directly with the increase in area.

3. The effectiveness of successive presentations of advertisements depends seemingly upon the interval of time between the presentations.

4. When the interval of time between successive presentations is very short, space used in advertising is most effective when broken up into small advertisements and presented in succession (according to Münsterberg).

5. When the interval of time between successive presentations is very long (a month), space used in advertising is more effective when used in a large advertisement than if presented in small advertisements and in succession.

Note.—Because of a number of practical considerations that must be taken into account the above conclusions do not apply equally to all phases of advertising. Direct application, then, can only be made when these practical considerations are kept in mind.

[1] More recent work of mine shows clearly that intervals of a few minutes or of a week are superior to that of a month. In the light of all the work on this subject, it seems probable that an interval of one day is such an optimum. See "Two Factors in Economical Learning," to appear shortly in the *Journal of Philosophy Psychology, and Scientific Methods.*

CONCERNING INDIVIDUAL DIFFERENCES IN REACTION TIMES

BY V. A. C. HENMON

University of Wisconsin, Madison, Wis.

AND

F. LYMAN WELLS

McLean Hospital, Waverley, Mass.

The fact of marked individual differences in reaction times and in types of reaction has often been noted. Less frequently has the question been raised as to the persistence of these differences after long-continued practice. Still less frequently if ever has the correlation between the times of simple and compound reactions been especially treated.

On the persistence of differences after practice, Alechsieff[1] says, "Individual differences in transit observations have been especially referred in part to practice and in part to the employment of different modes of reaction . . . . If the observers are thoroughly practised, personal differences become vanishingly small." Similarly, Whipple[2] says, "And now most important is this: when practice is attained, as indicated by a mean variation less than 10 per cent. of the average or median value, the results judging from my experience in the laboratory at least, are practically identical for all observers. In other words, our constant individual differences have disappeared since they themselves depend upon the degree of practice and upon variation in the direction of attention." These statements do not agree with the writers' experience with reaction time measurements. Individual differences both in simple and compound reactions persist after very long periods of practice and rest on more fundamental differences than those

[1] Alechsieff, 'Reactionszeiten bei Durchgangsbeobachtungen,' Philosophische Studien, 16, 1900, pp. 26–27.

[2] Whipple, G. M., 'Reaction-Times as a Test of Mental Ability,' *Am. Jour. of Psychol.*, Vol. XV., 1904, pp. 489–498.

of practice and direction of attention. There appears also to be a surprising lack of correlation between the times for simple and compound reactions.

In the study[1] of the time of perception as a measure of differences in sensations by one of the writers, long series of experiments on differences in color and pitch were made on two subjects, *H* and *W*. The average times for discriminative or choice reactions to red and yellow (440 cases) were as follows:

H	*W*
av. 216.9σ, m.v.17.8σ	av.256.5σ, m.v. 22.1σ

Before taking these series *H* had made approximately 2,000 simple reactions to sound and light stimuli and 3,000 discriminative or choice reactions to colors for the purpose of eliminating practice effects. *W* had made similarly a considerable but unknown number of simple reactions and approximately 1,500 compound reactions. From the time that the published experiments with the red and yellow were begun no appreciable differences in the times due to practice were discoverable. Similar differences between the times for the two subjects were shown in the experiments on pitch. The results for the differences of 16 vibrations (320 cases) were as follows:

H	*W*
av. 290.2σ, m.v. 26.6σ	av. 344.3σ, m.v. 40.9σ

After the completion of the study from which the above results are drawn *H* and *W* began certain studies of reaction times, only a portion of which was ever completed. Before these were begun *W* had made approximately 5,000 simple reactions (to visual stimuli) and 5,000 compound reactions; *H* approximately 4,000 simple reactions (visual and auditory) and 14,000 compound reactions. So far as it is possible ever to be sure in such matters practice effects had been entirely eliminated. The high degree of practice which both subjects had attained seemed to furnish an invaluable opportunity for a study of reaction times in general. The first experiments consisted of 1,000 simple muscular reactions to auditory stimuli, the Zimmerman sound hammer being used as stimulator.

[1] Henmon, 'The Time of Perception as a Measure of Differences in Sensations,' *Archives of Philosophy, Psychology and Scientific Methods*, No. 8, 1906.

Series of 100 reactions were taken each day. The results were as follows:

H	*W*
av. 125.4σ, m.v. 10.5σ.	av. 105.3σ, m.v. 10.5σ.

A contemporary research carried out independently in the same laboratory,[1] afforded data on these same individuals concerning the simple reaction times to visual stimuli of varying sizes. The averages of all reactions taken with each subject were as follows:

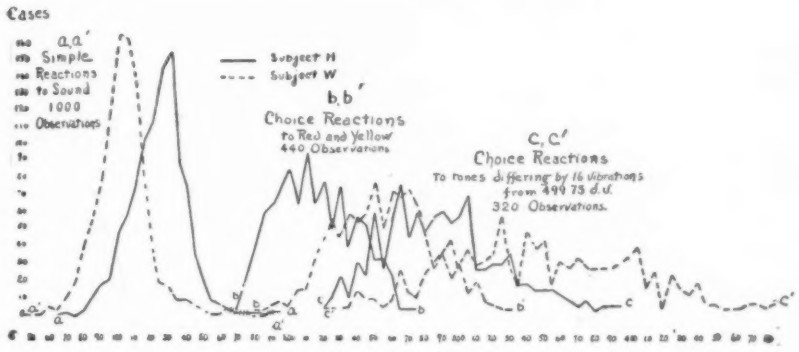
H

500 reactions, av. 184.6σ, m.v. 9.9σ

W

2,000 reactions, av. 173.6σ m.v. 8.6σ

The distribution of the observations in the preceding results with the exception of those for simple visual reactions, is shown graphically in the accompanying chart.



To render the curves more easily comparable, the scale in plotting the choice reactions is altered to make it correspond to that of the simple reactions.

In the two subjects, the simple reaction times differ with the auditory stimuli by 20σ, with the visual stimuli by nearly 11σ. Of the fact of these individual differences, preserved long after practice could essentially change them, there can be no dispute. This result is distinctly contrary to the statements of both Alechsieff and Whipple. It raises the question of whether the

[1] Froeberg, 'The Relation between the Magnitude of the Stimulus and the Time of Reaction,' *Archives of Psychol.*, No. 8, 1907.

subjects of either of these writers had had sufficient practice to permit of any generalizations concerning the properties of the simple reaction in advanced stages of practise. Neither adduces definite experimental evidence to this effect. The differences here found, are then, due neither to differences in practise, nor to differences in the direction of the attention (sensory or motor), since, so far as it is ever possible to control this factor subjectively, motor reaction alone was employed. The differences represent more fundamental differences in the speed of neural conduction—more probably a difference in the facility of synaptic coördination (or some other interneuronal process) than of transmission along nerve trunks.

Distinct individual differences also exist in the discriminative or choice reactions, but in the opposite direction from those of the simple reactions. Although reacting more quickly to simple light and sound stimuli, *W* reacts much more slowly where discrimination and choice between two stimuli are involved. The differences in times for the two subjects are 40σ for colors and 54σ for pitch. It should be noted also that false reactions in the discriminative or choice reactions were throughout nearly twice as frequent in *W* as in *H*. So far as it was possible to discover (we are now unable to give the exact figures), there was no essential difference in the sensory discriminativeness at the threshold of color or pitch. The significant feature of these results is the relatively greater speed of *W* in the simple reactions, combined with lower speed in the more complex ones. Since the greater speed of *H* in these latter reactions does not lie in more open sensori-motor arcs, nor in greater sensory discriminativeness, his results must be referred to more rapid conduction in the higher arcs directly concerned in the organic adjustment to these more complex situations. The independent variability between the simple and compound reactions furnishes strong evidence for the contention that the compound reactions are not merely simple reactions plus such processes as perception, apperception, discrimination, or choice. Hence, the times of these higher mental processes can not be secured, as is so often done, by simple subtraction.

Psychological Review Publications

PSYCHOLOGICAL MONOGRAPHS

VOL. III

11. *On Inhibition. B. B. Breese. Pp. iv + 65. 12. On After-images. Shepherd Ivory Franz. Pp. iv + 61. $1.12. 13. *The Accuracy of Voluntary Movement. R. S. Woodworth. Pp. vi + 114. 14. *A Study of Lapses. H. Heath Bawden. Pp. iv + 122. $1.50. 15. The Mental Life of the Monkeys. E. L. Thorndike. Pp. iv + 57. 50 cents. 16. *The Correlation of Mental and Physical Tests. C. Wissler.

VOL. IV

17. Harvard Psychological Studies, Vol. I.; sixteen experimental investigations. Edited by Hugo Münsterberg. Pp. viii + 654. $4.00.

VOL. V

18. Sociability and Sympathy. J. W. L. Jones. Pp. iv + 91. 75 cents. 19. The Practice Curve: J. H. Bair. Pp. 7[illegible] 75 cents. 20. The Psychology of Expectation. Clara M. Hitchcock. Pp. iv + 78. 75 cents. 21. Motor, Visual and Applied Rhythms. J. B. Miner. Pp iv + 106. $1.00 22. The Perception of Number. J. F. Messenger. Pp. iv + 44. 50 cents. 23. A Study of Memory for Connected Trains of Thought. E. N. Henderson. Pp. iv + 94. 75 cents.

VOL. VI

24. A Study in Reaction Time and Movement. T. V. Moore. Pp. iv + 86. 75 cents. 25. The Individual and his Relation to Society. J. H. Tufts. Pp. iv + 58. 50 cents. 26. Time and Reality. J. E. Boodin. Pp. v + 119. $1.00. 27. The Differentiation of Religious Consciousness. Irving King. Pp. iv + 72. 75 cents. 28. University of Iowa Studies. No. IV. Edited by C. E. Seashore. Pp. v + 118. $1.25.

VOL. VII

29. Yale Psychological Studies, New Series, Vol. I. No. 1. Edited by Charles H. Judd. Pp. vii + 226. $2.25. 30. The Theory of Psychical Dispositions. Charles A. Dubray. Pp. vii + 170. $1.50. 31. Visual Illusion of Movement during Eye Closure. Harvey Carr. Pp. vi + 127. $1.25.

VOL. VIII

32. The Psychological Experiences connected with the Different Parts of Speech. Eleanor H. Rowland. Pp. 42. 40 cents. 33. Kinæsthetic and Organic Sensations: Their Role in the Reactions of the White Rat to the Maze. John B. Watson. Pp. vi + 100. $1.00. 34. Yale Psychological Studies, New Series. Vol. I. No. 2. Edited by Charles H. Judd. Pp. v + 197. $1.75. 35. Studies from the Psychological Laboratory of Wesleyan University. Vol. I. No. 1. An Experimental Study of Visual Fixation. Raymond Dodge. Pp. vii + 95. $1.00.

Note.—No. 36 appears as No. 1 of the Philosophical Monographs.

VOL. IX

37. Studies from the Psychological Laboratory of the University of Chicago. Control Processes in Modified Hand-Writing; An Experimental Study. June E. Downey. Pp. vii + 148. $1.50. 38. University of Iowa Studies in Psychology. No. 5. Edited by Carl E. Seashore. Pp. 148. $1.50. 39. Studies from the Psychological Laboratory of the University of Chicago. Combination Tones and Other Related Auditory Phenomena. Joseph Peterson. Pp. xiii + 136. $1.50.

VOL. X

40. **Studies from the Johns Hopkins Psychological Laboratory.** Edited by G. M. STRATTON. Pp. 104. $1.00. 41. **The Social Will.** EDWIN ANDREW HAYDEN. Pp. iv + 93. $1.00. 42. **Studies from the Psychological Laboratory of the University of Chicago. The Effect of Achromatic Conditions on the Color Phenomena of Peripheral Vision.** GRACE MAXWELL FERNALD. Pp. iv + 91. $1.00. 43. **Wellesley College Studies in Psychology, No. 1. A Study in Memorizing Various Materials by the Reconstruction Method.** ELEANOR A. MCC. GAMBLE. Pp. xi + 211. $2.25.

VOL. XI

44. **Studies from the Psychological Laboratory of the University of Illinois. Vol. I. No. 1.** Edited by STEPHEN S. COLVIN. Pp. vi + 177. $1.75. 45. **Ohio State University, Psychological Studies. Vol. I. No. 1.** Edited by THOMAS H. HAINES. Pp. 71. 75 cents. 46. **Studies from Psychological Laboratory of University of Chicago. An Experimental Study of Fatigue.** C. S. YOAKUM. Pp. vi + 130. $1.25. 47. **Studies from the Johns Hopkins Psychological Laboratory. The Determination of the Position of a Momentary Impression in the Temporal Course of a Moving Visual Impression.** N. T. BURROW. Pp. 63. 65 cents.

VOL. XII

48. **A Study of Sensory Control in the Rat.** FLORENCE RICHARDSON. Pp. 124. $1.25. 49. **On the Influence of Complexity and Dissimilarity on Memory.** HARVEY A. PETERSON. Pp. 86. $1.00. 50. **Studies in Melody.** W. VAN DYKE BINGHAM. Pp. vi + 88. $1.00. 51. **Report of the Committee of the American Psychological Association on the Teaching of Psychology.** Pp. 94. $1.00. 52. **Some Mental Processes of the Rhesus Monkey.** WILLIAM T. SHEPHERD. Pp. 66. 75 cents.

VOL. XIII

53. **Report of the Committee of the American Psychological Association on the Standardizing of Procedure in Experimental Tests.** Pp. 108. $1.00. 54. **Tests for Practical Mental Classification.** WILLIAM HEALY and GRACE MAXWELL FERNALD. Pp. viii + 54. 75 cents. 55. **Some Types of Attention.** H. C. MCCOMAS, JR. Pp. 56. 75 cents. 56. **On the Functions of the Cerebrum: the Occipital Lobes.** SHEPHERD IVORY FRANZ and GONZALO R. LAFORA. Pp. 118. $1.25. 57. **Association Tests: Being a Part of the Report to the American Psychological Association of the Committee on Standardizing Procedure in Experimental Tests.** R. S. WOODWORTH and F. LYMAN WELLS. Pp. 86. 75 cents.

VOL. XIV

58. **The Diagnosis of Mental Imagery.** MABEL RUTH FERNALD. Pp. 160. $1.50. 59. **Autokinetic Sensations.** HENRY F. ADAMS. Pp. 45. 50 cents. 60. **A Study of Cutaneous After-Sensations.** MARY H. S. HAYES. Pp. 89. $1.00. 61. **On the Relation of the Methods of Just Perceptible Differences and Constant Stimuli.** SAMUEL W. FERNBERGER. Pp. 81. $1.00.

VOL. XV

62. **The Factors that Influence the Sensitivity of the Retina to Color.** GERTRUDE RAND. Pp. 178. $1.75. 63. **Learning in Dementia Precox.** EDWIN G. BORING. Pp. 101. $1.00. 64. **An Experiment in Linear Space Perception.** FRANCIS N. MAXFIELD. Pp. 56. 75 cents. 65. **The Form Board Test.** RENEL HULL SYLVESTER. Pp. 56. 75 cents. 66. **The Influence of Stimulus Duration on Reaction Time.** G. R. WELLS. Pp. 68. 75 cents.

PHILOSOPHICAL MONOGRAPHS

VOL. I

1. **Æsthetic Experience: Its Nature and Function in Epistemology.** W. D. FURRY. Pp. xvi + 160. $1.60. 2. **The Philosophy of John Norris of Bemerton.** FLORA ISABEL MACKINNON. Pp. iii + 103. $1.00.

Price $4.00 the volume. *Monographs so marked are out of print.